THURSDAY, FEBRUARY 26, 1885

THE RELATIVE EFFICIENCY OF WAR-SHIPS

THE *Times* of the 19th inst. contains a long and vigorous criticism by Sir E. J. Reed, M.P., of the ten largest British ships of war "launched in 1879, or since, or remaining on the stocks." These are the *Ajax*, *Agamemnon*, *Colossus*, *Edinburgh*, and the six vessels which constitute the *Admiral* class. These vessels are all built upon the central citadel system—*i.e.* their armoured portions are merely citadels erected in the middle of the length; the ends being left without armour-plating. One of these ships may thus be considered as being divided into three parts, so far as her out-of-water structure is concerned. The central part is plated completely around with very thick armour, which extends from the upper deck to several feet below the water-line; while the parts before and abaft this are not protected by armour, but rest upon a thickly plated deck situated at the depth of the lower edge of the citadel armour. This deck protects the hull beneath the armour against the effects of a plunging fire.

This system of construction was advocated by Sir E. J. Reed before the Committee of Naval Designs in 1871. It was first adopted in the *Inflexible;* and immediately gave rise to a discussion respecting the size of the armoured citadel which Sir E. J. Reed has, with persistent energy, kept up ever since. The *Times'* letter above referred to is a continuation of the old, and well-remembered, *Inflexible* debate. A statement of the points then in dispute will be found in NATURE of July 12 and 19, 1877. Sir E. J. Reed maintained that the fighting power of the *Inflexible* was gravely compromised by the shortness of her armoured citadel—which was not long enough to make the ship stable in the event of her thinly plated ends being so much injured as to lose all power of excluding water. A committee was appointed to inquire into and report upon the matter, but Sir E. J. Reed refused to give evidence before it.

Sir E. J. Reed now says, with reference to the later ships of this type: "I have to state, and am prepared to demonstrate to any competent tribunal, that there is not one of these ten ships, the latest added to the British Navy, that cannot be either capsized and sunk, or sunk without capsizing, without any shot or shell whatever being directed against those parts of the ship which are armoured. . . . The French armoured ships . . . must in all reason be expected to dispose of these English ships in a very few minutes by simply destroying their unarmoured parts. . . . I will here repeat in the most public and responsible manner that the *Ajax*, *Agamemnon*, *Colossus*, and *Edinburgh*, and the six ships of the *Admiral* class, are all utterly unfit to engage the corresponding French ships; unfit to enter the line-of-battle at all; and unfit to be retained on the list of armoured ships."

This is strong language, but not so strong as that which is used respecting the members of the Board of Admiralty and the Constructors of the Navy. Sir E. J. Reed blames Admiral Sir Cooper Key, the First Sea Lord of the Admiralty, for not setting his face against "the prospect of British ship after ship capsizing in battle, before their armour had been violated or touched." He fears that the day may be near "when the present betrayal of our Navy by a set of politicians, admirals, and constructors may wring from us a cry which the very ends of the earth will hear." The Admiralty of the day is "foolish enough, cruel, heedless, reckless, and faithless enough" to rely upon the skill and vigilance of the seamen "whom they send unprotected to destruction"; and "to substitute them for those actual physical defences which the ship herself should embody." Sir E. J. Reed is "fast coming to feel something very like contempt" for the heads of the Admiralty; and he considers that "they are unequal to the work they have undertaken, and have become a source of grave national danger. . . . Upon the heads of the present Board of Admiralty must continue to rest, after this public warning, the responsibility of delivering ten British ships of the largest class an easy and certain prey to destruction should war arise."

These are grave charges; and if the questions involved by them could be settled by forcible or scornful language, there would be little remaining to be said. It is desirable, however, to disregard as much as possible the rhetorical effect of the statements made, and to endeavour to ascertain what are the simple facts of the case. It is important likewise to remember that the comparison instituted between our ships and those of the French is not one between fully armoured and partially armoured ships, but between partially armoured ships on both sides. The armour protection is very limited in the French ships, but it is differently distributed from what it is in ours. The armour of the French ships stops at a very small height above the water-line: and the space between the top of the armour and the upper deck may be destroyed as easily as the unarmoured ends of our ships. Any approach to destruction would completely cripple the fighting power, speed, and manœuvring qualities of these ships.

If the assumptions upon which Sir E. J. Reed's main argument is based are sound and indisputable, then no condemnation of the Board of Admiralty and of the Naval Constructors could be too strong or unqualified. We are disposed to go a long way with him in believing that all is not so well as might be wished with our recent ships, and that there is incompetency and something very like indifference to be found in high quarters at the Admiralty: but, before adopting, in all their breadth and fullness, the views so vigorously and ably advocated by Sir E. J. Reed, there are one or two points upon which we feel that more light is needed. Indeed, we are convinced that the present widely discordant views that are held by the different parties to this naval discussion are impossible of reconciliation until the points referred to are cleared up.

The chief one of all is, Can the thinly-plated ends of these citadel ships be readily destroyed in action and made useless—or worse than useless—for the purpose of contributing buoyancy or stability to the ship? If they can, it is obvious that the ship's safety may be speedily endangered without the thick armour plating of the citadel being penetrated. Sir E. J. Reed assumes that this is unquestionably the case, and he emphatically asserts that our ten most powerful ships of recent construction might

be disposed of "in a very few minutes by simply destroying their unarmoured parts." It is upon this assumption that his charges against our ships and their constructors are mainly based. If it be correct, the Admiralty stand convicted of culpable neglect or error; but if it be incorrect, or very doubtful, then Sir E. J. Reed's charges are pointless and unjustifiable.

The question is one of most vital importance to the fighting efficiency of our principal ships of war; but how is it to be settled? It is not one with which mere theory or abstract science can deal: actual experiment can alone answer it. Sir E. J. Reed believes, and asserts, that such structures as the thinly-plated ends of our recent ironclads may be effectually destroyed in a few minutes, and that single shells may shatter large portions of them into fragments. He says:—"It is not a mere question of riddling the ends, but also one of blowing them up by shell fire: and how effectually they may be thus destroyed was shown at Alexandria, where a single shell, bursting against the unarmoured part of the *Superb's* side, tore a hole in it 10 feet by 4 feet in extent."

The apologists for this system of construction say, on the other hand, that if the area is increased over which the armour is spread, as would be the case if the citadels were lengthened, the thickness of armour throughout would require to be reduced; and the armour protection would therefore be less in the central portion of the ship which incloses the boilers, engines, and other essential elements of fighting efficiency. Many naval artillerists say, further, that unless the ends can be plated with the very thickest of armour, it is better to include everything which contributes to fighting power within the armoured citadel or below the armoured deck, and to make the ends as thin as possible. They argue that shells which meet with considerable resistance in penetrating armour of moderate thickness will shatter the ship's side, and make holes which cannot be stopped; whereas they almost invariably make clean holes through thin plating, and would, in the vast majority of cases, pass through the ship and out upon the other side. Such an instance as Sir E. J. Reed calls attention to in the case of the *Superb* would not, it is said, occur in practice more than once in one hundred times. The clean holes made by shells in thin plating can be stopped effectually and quickly by men stationed inside with shot-hole stoppers. These are made of india-rubber, and open and close like an umbrella. They are pushed out from the inside, and then pulled back and opened over the outside of the hole. The buoyancy and stability afforded by the ends can, it is confidently stated, be preserved by these means; whereas the damage done to any but the very thickest of armour plating would be so much greater that the holes made by shells could not be so effectually dealt with.

It is also pointed out that it is extremely difficult to strike a ship exactly at her water-line. The great majority of projectiles strike at some distance above it. If they are aimed too low they ricochet from the water surface and strike the ship above the water-line. It is most difficult to penetrate a ship exactly at her water-line; and if she is so penetrated, the holes may be much more readily and effectually stopped when the plating is thin than when it is thick. This is the argument which forms the answer to Sir E. J. Reed's charges.

Sir E. J. Reed says that "the reply to the British ships which are being made to depend for their flotation and stability upon their unarmoured ends will inevitably be small-gun attack," and he considers that even the fire from machine-guns may be sufficient to cripple them. This opens up a complicated question and one which cannot be fully considered in all its details from a merely abstract point of view. There is obviously, however, a limit to the effective use of small gun and machine-gun fire, which is imposed by the necessity of protecting them by armour if they are to fight at short range. If the guns are not protected by armour they can only be relied upon at long ranges; and even then they may as readily be placed *hors de combat* by the fire from the enemy as succeed in penetrating, still less in destroying, the unarmoured ends of the latter.

These are points which experience alone can throw any clear and definite light upon. Each party may continue to advocate its own view with great show of reason, but neither will convince the other till the effect of artillery fire upon such structures as the unarmoured ends of the ships in question has been thoroughly tested. In the meantime the public mind is only being bewildered and wearied by the reiterated discussions of questions which cannot be settled by mere argument or force of words.

A structure similar to the unarmoured ends of one of our ships might easily be built and placed afloat. It should then be fired at from various distances with guns of different sizes. Valuable data might then be obtained upon two crucial points: (1) the percentage of shots which would strike sufficiently near the water-line to affect prejudicially the buoyancy or stability; and (2) the nature of the holes that would be made; whether such as are capable of being easily stopped from the inside, or such as admit of no effectual stoppage, but practically constitute a disintegration or destruction of the fabric. This simple experiment might surely be made in such a way as to set at rest the discussion that has now been going on for so many years respecting the efficiency of the system upon which the safety and fighting power of our most powerful ships depends. Still, "water conditions" would be the most favourable for such experiments; because it would obviously be more difficult to make good practice at a vessel's water-line in action—under the ordinary circumstances, at sea, of rolling motion and the relative movements of the vessels engaged—than at a quiet and carefully arranged trial.

The only logical and effective answer that can be made to Sir E. J. Reed's letter is that which would be furnished by the results of experiments such as we have indicated; and that answer cannot be made too soon, or too complete, either for the reputation of the Admiralty and of the Constructors of the Navy—who, to say the least, appear to be greatly in the dark respecting the practical merits of the system to which they are committed—or for the satisfaction of the public mind.

This question, upon the merits of which Sir E. J. Reed's charges must either stand or fall, is one which only Science can settle by experimental tests; but there is an important point underlying another assumption contained in his letter which may be discussed with advantage from a more abstract point of view. He says: "The Admiralty Director of Naval Construction, in the article 'Navy,' in the 'Encyclo-

pædia Britannica,' lays down the following principle:—'The fairest available approximate measure of the power of the ships is their displacement or total weight. It always represents power of some kind.'" Sir E. J. Reed adopts this principle, without reserve or qualification, and employs it as an empirical method of determining the relative fighting powers of the ships of our own and the French navies.

"Bearing this principle in mind, as one accepted and avowed by the Admiralty" he proceeds to compare the displacements of ten of the largest French ships recently built with ten of the corresponding ships of our own navy. The following result is arrived at:—"Looking at these figures, and bearing in mind the doctrine quoted—that superior displacement means superior power, and inferior displacement inferior power—we here see that the English ships have been deliberately made inferior by our Admiralty, ship by ship and squadron by squadron."

We do not know what authority there is for saying that this "principle" is accepted and avowed by the Admiralty. True, it is propounded by Mr. Barnaby, the Director of Naval Construction, in the latest edition of the "Encyclopædia Britannica"; but we have not heard that the Admiralty accept and avow it. We hope, for the sake of the scientific reputation of the Naval Department, that they hold no such fallacious and absurd doctrine. It is surprising to find a scientific man of Sir E. J. Reed's eminence and ability assenting to, and adopting, Mr. Barnaby's so-called "principle." What is stranger than all, however, is that Sir E. J. Reed should not see that the adoption of it is inconsistent with his main contention that our ten newest armour-clads are practically worthless, for quite other reasons, as compared with those of the French, and could be disposed of by the latter "in a very few minutes."

The average displacement of the ten English ships referred to by Sir E. J. Reed is 9,363 tons, and that of the corresponding ten French ships is 10,470 tons. Applying Mr. Barnaby's principle in the sense in which it is used by Sir E. J. Reed—bearing in mind that "superior displacement means superior power, and inferior displacement inferior power," and that "the fairest available approximate measure of power" is "displacement or total weight"—we arrive at the conclusion that the fighting power of the ten English ships is rather less than nine-tenths that of the French ships. Had their displacements been greater they would, upon the same principle, have been more powerful than the French ships. But Sir E. J. Reed believes that, apart from displacement altogether, and because of the different systems of construction employed in the two cases, the English ships could be sunk by the French ships in a very few minutes. The assumptions upon which the respective arguments are based are obviously inconsistent with each other. One is that the English ships are inferior to those of the French because their displacements are less; the other is that they are inferior because the details of their construction are not so wisely and efficiently designed. Either one or both assumptions may be correct; but the one has no necessary relation to the other.

But we will compare Mr. Barnaby's present principle with an empirical formula previously laid down by him for determining the comparative efficiency of ships of war. In the course of a lecture delivered in the Royal United Service Institution, in 1872, upon "Modern Ships of War," Mr. Barnaby put forward the following formula:—

$$\frac{A \times G \times H \times S^3}{L \times 100} = \text{comparative efficiency},$$

where A is the weight of armour per ton of ship's measurement, G the weight of protected guns and ammunition, H the height of battery port-sills above load water-line, S the speed in knots at the measured mile, and L the length of the ship.

Mr. Barnaby applied this formula to the seven ironclads named in the table given below. In this table we have placed, alongside the names of the vessels, a column which contains their displacements in tons. The next column contains their comparative efficiencies, as computed by the above formula; and the last column contains their comparative efficiencies, upon Mr. Barnaby's new principle that displacement is a fair measure of power. It will be seen that, according to the latter, the most powerful of these seven ships is the *Minotaur*, and the next the *Warrior*. The relative efficiency of the former vessel is three times greater than that given by Mr. Barnaby's previous formula; and the latter is nearly four times greater. The *Warrior* and the *Minotaur* are, according to this standard of comparison, the most powerful of the seven ships named; while the *Minotaur* would, upon the same principle, be classed as the most powerful fighting ship the British navy possesses at the present time—with the single exception of the *Inflexible*. In reality, however, the *Warrior* and *Minotaur* are the weakest and least efficient ironclads we possess; and are invariably classed as obsolete even in the most favourable estimates that are made of the fighting power of the British navy.

Names of ships	Displacement in tons	Relative efficiencies as computed by Mr. Barnaby's formula, $\frac{A \times G \times H \times S^3}{L \times 100}$	Relative efficiencies upon principle that power varies with displacement
Monarch	8,320	149·8	149·8
Hercules	8,680	113·4	156·2
Captain	7,900	83·3	142·2
Vanguard	6,010	83·0	108·2
Minotaur	10,690	61·1	192·4
Warrior	9,210	44·5	165·8
Defence	6,150	10·9	110·7

Nothing further can be necessary to show the fallacy, and the absolute inconsistency, of the views put forward at various times by Mr. Barnaby, respecting the standard by which the fighting power of a ship, or of a navy, may be judged. He has given no justification of either of the methods described; nor attempted to show that they are approximately reliable. The formula laid down by him in 1872 recognises that the fighting power of a ship of war is made up of various distinct and independent elements—that the amount of armoured protection, as represented by weight of armour; the power of the armament, as measured by its weight; the speed, and other qualities constitute elements of fighting power, which have different relative values, and which must be separately taken into account. We here find the value of manœuvring power, or handiness in turning, recognised by introducing the length of the ship as a divisor into the formula. This element of fighting power is assumed to

vary inversely as the length; so that, in similar ships, it would vary inversely as their displacements. In other words, so far as one element of fighting power is concerned, and that a very important one, the measure of its amount is not the displacement, as Mr. Barnaby now assumes, but the inverse ratio of the displacement.

The fighting power of a ship is thus composed of several diverse and independent elements; and there is nothing approaching to a consensus of professional opinion as to the relative importance of these elements. To assume that they all vary together with the ship's dimensions, or with her weight in tons, is in the highest degree delusive and absurd. The displacement of a ship measures her weight and nothing more. Whether that weight has been effectively and wisely employed in developing a high degree of fighting power, is an entirely independent matter; and one upon which the whole question of fighting efficiency depends. The statement that displacement "always represents power of some kind," merely begs the question. Of course it represents power; but such power is simply that of displacing water. It may represent that and nothing more, or it may represent in addition the possession of great fighting power, or of other desirable qualities. But the possession of such qualities, and the degree in which they will be developed, must depend entirely upon the skill of the designer—an arbitrary personal factor which is not always limited by the cubic feet of displaced volume that are placed at his disposal. Mr. Barnaby himself pointed out in the paper above referred to, that although the *Defence* and *Vanguard* have approximately equal displacements, the latter carried one-half more armour-plating than the former upon three-fourths of the weight of hull; and was so superior in manœuvring capability that she would turn completely around in four and a half minutes, whereas the former vessel required seven minutes to complete a circle. This difference in qualities, and superiority in fighting power, of the *Vanguard* over the *Defence* is absolutely undiscoverable by merely comparing the displacements.

All the comparisons we have seen of the fighting powers of modern ships of war and of our own and foreign navies, have been more or less vitiated by the arbitrary standards that have been selected as the basis of such comparisons. The displacement basis is unreliable and misleading, and furnishes no test whatever of fighting power. It would be extremely difficult to devise any simple standard by which the popular mind may be fairly impressed with the relative powers of our own and foreign navies; while for purposes of exact comparison or of technical discussion no such standard could be regarded as absolute. Before a simple standard or unit of comparison can be framed, which will be satisfactory or useful, naval officers, artillerists, and constructors require to agree among themselves about the relative importance of the various elements that make up the fighting power of a ship. The defensive values of armour-plating, speed, turning-power, and other protective qualities, and also the offensive values of the gun and torpedo armaments, the ram, speed, &c., require to be separately evaluated and their relative importance determined. If a general agreement could be arrived at as to the relative approximate values of each of these independent elements of offensive and defensive power, an empirical formula might be framed —such as Mr. Barnaby attempted with insufficient data in 1872—which would fairly represent the gross fighting efficiency of a ship. Till this is done, no rule can possibly be devised which will indicate anything more than the mere opinions of the person who frames it; while often, as in the case of Mr. Barnaby's present displacement basis, the application of the rule may be misleading in a degree which its framer could never have foreseen or intended.

Sir E. J. Reed's letter to the *Times*, and the whole force of the charges contained in it, rests mainly upon the truth of the two assumptions we have considered. The first is that the unarmoured ends of our present ironclads have practically no protective value. This is a point which, as we have said, may be determined once and for all by scientific experiments. The second assumption is that the comparative efficiency of our own ships and those of foreign powers may be approximately measured by merely comparing their displacements. This proposition is unsound, and does not admit of any qualifying corrections short of depriving it of all specific meaning. A scientific standard or unit of comparison which may be fairly applied to the approximate determination of the relative fighting powers of war-ships and navies is greatly to be desired; but before such an one can be framed, the persons who have to use our ships of war and to take them into action, and those who are responsible for their efficient construction, must come to some definite understanding as to what the various elements of fighting power consist of, and what are their relative degrees of importance; and to do so they must call in the aid of Science.

PROFESSOR WILLIAMSON'S DYNAMICS

An Elementary Treatise on Dynamics, containing Applications to Thermodynamics, &c. By Benjamin Williamson, F.R.S., and Francis A. Tarleton, LL.D. (London: Longmans, Green, and Co., 1885.)

PROFESSOR WILLIAMSON is already so well known to the student by his excellent text-books of the Differential, and of the Integral, Calculus, that his appearance in a new field of authorship is sure to excite attention. We accordingly opened the present work with expectations of a very high order. Not, of course, expectations that much novelty of matter could be introduced in an elementary work on a subject which has been thoroughly threshed-out, but that possibly fresh interest and easier assimilability might be given to long-known facts and processes by some novel mode of presentation.

In these expectations we have been disappointed. Either the subject of Dynamics does not admit of treatment superior to that which it has already received, or our authors are not destined to be the pioneers to the possible improvements. Our special reasons for this statement we will give with some detail, but we may begin with some general observations.

From the time in which Jackson, Lloyd, Whewell, and many others, introduced continental methods to the average Honour-man; through the period of Earnshaw, Pratt, Wilson, Tait and Steele, Griffin, Walton, &c., to the Parkinson, Bezant, Routh, &c., of the present day, there has been a plethora of treatises in English on the various parts of elementary Dynamics. Some of these

were robust, and showed considerable vitality, others sickly and short-lived. But, bad or good, among them they have practically exhausted the resources of the subject, so far as the theorems presentable to a beginner are concerned. The only ringing of the changes has been in arrangement, modes of presentation, and proofs.

But from the books of the future, some of which, at least, we may expect to see starting into existence in the present, we naturally, though perhaps vainly, look for something higher and better than this. We now have elementary treatises on the various branches of mathematics required in Dynamics (two, in fact, due to Prof. Williamson himself) so much superior to any that existed even twenty years ago, that we no longer require to have intricate steps of ordinary differentiation or integration introduced into a text-book of that subject. What we require may be summed up in two words, *Foundation* and *Arrangement*. To these must, of course, be added, as a requirement in every scientific treatise, *Consistency*.

The foundations of the subject, in by far the best form in which they have yet been presented, were given by Newton. He expressly states, before proceeding to give his second interpretation of the Third Law of Motion, that (so far) he had been giving principles generally accepted among mathematicians. But we can barely imagine the effort which must have been made by that transcendent genius in extracting such simple and yet all-comprehending statements from the portentous verbiage of even the most able of his precursors. Step by step, in Britain, Newton's system was forsaken; one of his Laws was split up into fragments, another ignored and its place supplied by gratuitous additional Axioms; till at last the monstrous process culminated in the adoption of Duchayla's so-called statical *Proof* of the Parallelogram of Forces. Thus everything was ripe for Thomson and Tait's reintroduction of the grandly simple system of Newton. The results of this step have been alike remarkable and important. These authors also introduced, after the example of Ampère,[1] the notion of separating the science of motion in the abstract (*Kinematics*) from that of motion of matter:—thus lightening the student's work, in Dynamics proper, to at least as great an extent as it is lightened by his previous study of integration and differential equations.

Now, in the book before us, these improvements on the text-books of twenty years ago are only partially adopted. Kinematics is not made a strictly preliminary study, but inserted in detached fragments. The exploded "statical measure" of force haunts us all through the book, sometimes leading to extraordinary results. Thus, opening at p. 30, we find the following passages, in which we have italicised a few words:—

"Acceleration varies as Pressure."

"This equation enables us to determine the velocity generated . . . by a constant force . . . whenever the *pressure* which measures the *force* is known, and also the *weight* of the body."

"Thus a force which is capable of supporting a weight of 112 lbs. is called a force of 112 lbs."

". . . the same *effort* which would project a small stone to a considerable distance will move a large one but slightly."

[1] Ampère has never, to our knowledge, received the credit due to him for much of his best dynamical work:—*e.g.* the u, θ equation of central orbits.

Here we see, at a glance, the effects of want of system Pressure, Force, and Effort are used as completely synonymous and interchangeable terms. Now the first term has a perfectly definite meaning in science (introduced without definition or warning by our authors in § 290 of the book, to the utter bewilderment of the reader fresh from p. 30), and it means something differing from force in exactly the same way as a linear inch differs from a cubic inch. As to the Effort exerted in throwing a stone, we imagine that, if employed at all in scientific language, it would signify properly the work done, not the force applied; the two things differing as a square foot does from a linear foot. Of course our authors do not require to be told this, but why muddle the student by giving him slipshod information which he must ***unlearn***, if he is ever to make progress?

On the opposite page (31) we find:—

"If a uniform pressure [force] of 3 lbs. [weight] produce a velocity [speed] of 10 feet [per second] in the first second, find the weight [mass] of the body acted on."

The insertions are ours, made with the view of showing how the question ought to be stated unless there is to be complete confusion of nomenclature.

Since Clerk-Maxwell published his admirable little book on "*Matter and Motion*" there has been left no excuse whatever for a misuse of the word *Velocity*. The adoption of Hamilton's Vector ideas effected an immense improvement in all these elementary matters. Yet we not only find constantly, in the book before us, this confusion of speed and velocity, but something even more grave, of which one example appears in the above extract. This is the use of the word "velocity" in the sense of so many units of length. See, for instance, pp. 28, 29:—

"'In what time will a falling body acquire a velocity of 400 feet?'

"'If one minute be taken as the unit of time, what should be taken as the value of g?'

'*Ans.* The velocity per minute acquired in one minute by a falling body.'"

Now, what on earth is a "velocity of 400 feet" or a "velocity per minute"? To make the first statement intelligible we must add "per (specified unit of time)"; and for "velocity," in the second statement, we must read "velocity in feet"; or, preferably, "speed in feet." The "per unit of time" is already present on *this* occasion.

Under this category we must quote the truly sensational heading of § 19:—

"Relation between Velocity and Space,"

for this is also obviously based upon the above erroneous designation of "velocity" as so many units of length.

In p. 124 we find:—

". . . time becomes a necessary element when we come to compare the *efficiency* of different agents. For instance, if one agent . . . performs an amount of work in one hour which it requires another five hours to accomplish, the former is said to be five times as efficient." [The italics are in the text.]

But, turn to p. 438, and we read:—a heat-engine being now the "agent":—

". . . the ratio of the heat converted into work to the heat drawn from the source is called the *efficiency of the engine*." [Again, the italics are in the text.]

It appears from this, as from a former example, that it is necessary to take the same word in two perfectly different meanings according as it is met with in the first (or ordinary dynamical) part of the book, and in the later (or thermodynamical) part. Such at least is the case with the two specially important terms, *Pressure* and *Efficiency*.

It is perhaps hypercritical to call attention to peculiarities of expression which, however they may puzzle him, can scarcely mislead the student. Else we might ask why (p. 8) a point is "*animated* by any number of velocities," or "*subjected* to any number of simultaneous velocities," or why "additional velocity" is said in contrast (p. 12) to be "*received*."

We have marked at least a score of places, in addition to those already noticed, in which the same or similar confusion occurs:—and yet we have *read* in all only about a fourth of the book here and there, having glanced over the rest much more hastily. But it is enough to have said, while illustrating our remarks by simple instances, that this is certainly not a book for beginners, nor for any one whose hold of the exact meaning of scientific terms is precarious:—though it may be consulted without danger (scarcely, we should think, with actual pleasure) by a student who, already soundly educated in the *principles* of Dynamics, desires to get a rapid and condensed *résumé* of their development by mathematical methods.

The principle of dual authorship rarely works well in practice. One of the authors of this book invariably speaks of *Centre of mass* (or of *inertia*) of a body, the other as invariably of *Centre of gravity*. And their responsibility has been so thoroughly divided, that *neither* of these terms is defined, so far as we can find (even with the help of the Index), anywhere in the volume. Again, one of the authors seems to have been always on the look-out to put in a little bit of Kinematics wherever he had a chance. And surely a third must have been at work, whose function was to stick in some sections on the *Rotation of a Rigid Body* (p. 92) between the sections on *Circular Orbits* and those on the *Simple Pendulum*.

The extraordinary *Olla podrida* of Schell is one of the authorities mentioned in the *Preface* as having been largely borrowed from. The book would certainly have been very much better had that work been let alone; though no work more richly deserves to be plundered in its turn than does that of Schell, who simply adopts (and too frequently distorts) whatever pleases him.

OUR BOOK SHELF

Les Organismes problématiques des Anciennes Mers. By the Marquis de Saporta. (Paris: Masson, 1884.)

THE views expressed in Saporta and Marion's "Evolution des Cryptogames" (reviewed at length in NATURE, vol. xxiv. pp. 73, 558) as to the origin of certain markings commonly met with in palæozoic rocks, has led to a long discussion in which many have taken part, the chief champions on either side being Dr. Nathorst, the distinguished Swedish botanist, and the Marquis de Saporta. Dr. Nathorst maintains that they are tracks left by moving or burrowing animals or other inorganic markings, whilst Saporta holds to his original opinion that very many of them are casts of primæval *algæ*, of kinds now extinct. Nearly all of these markings are in bas-relief on the under surfaces of slabs as if they were moulds of prints or impressions traced in the ancient muds, thus at first sight greatly favouring Nathorst's view of their origin. Saporta demonstrates on the other hand that this is a by no means uncommon mode of fossilisation among undoubted plants, and when we reflect on the composition of algæ, we shall see that scarcely any other mode of fossilisation among them is possible. A leathery olive green sea-weed lying on an oozy mud would cause an indentation, and if subsequently covered up, would keep the old surface from contact with the fresh mud, until it might, under favourable conditions, have become sufficiently hardened to retain the impression. The sea-weed, as most olive weeds do now, if left in water or fresh mud, would eventually completely dissolve away, leaving no perceptible organic trace of its presence. The cavity thus left would be filled in at last by the overlying mud, and only a cleavage plane would remain, following the contour of the under side of the weed, and marking its former presence. Sometimes, though rarely, the sea-weed might not decay until a cleavage plane had been established around its entire circumference, without leaving the smallest trace of its internal structure, as we often find is the case with far more resisting cryptogamic stems in the older rocks. This Saporta finds is the case with the Bilobites, one of the most vexed of all the "Organismes problématiques," and he relies with good reason upon their occasional occurrence in this condition and on their reticulated structure to support his contention that they cannot be mere worm tracks or burrows, and that in point of fact they can be naught but the impressions of primordial algæ.

J. S. G.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Civilisation and Eyesight

MY attention has only recently been called to a communication from Lord Rayleigh, which appears in NATURE for the 12th inst. (p. 340), and on which I crave permission to make a few observations. Lord Rayleigh questions whether the eyes of savages, "merely as optical instruments," are greatly superior to our own; and suggests that any superiority which savages possess may depend upon "attention and practice in the interpretation of minute indications." He explains that "the resolving power of an optical instrument is limited by its aperture," and then proceeds as follows:—

"With a given aperture no perfection of execution will carry the power to resolve double stars, or stripes alternately dark and bright, beyond a certain point, calculable by the laws of optics from the wave-length of light. With sufficient approximation we may say that a double star cannot be fairly resolved unless its components subtend an angle exceeding that subtended by the wave-length of light at a distance equal to the aperture. If we take the aperture of the eye as one-fifth of an inch, and the wave-length of light as 1-40,000th of an inch, this angle is found to be about two minutes; and we are forced to the conclusion that there is no room for the eye of the savage to be much superior in resolving power to those of civilised physicists, whose powers approach at no great distance the theoretical limit as determined by the aperture."

I understand this to mean that optical conditions limit the resolving power of the eye to objects which subtend a visual angle of about two minutes, and that civilised physicists approach this theoretical limit at no great distance.

With great submission to the high authority of Lord Rayleigh, I venture to question whether we have any data from which to draw conclusions with regard to the possible optical powers of the eyes of the human race. We should probably fall into grave error if we were to argue from the reduced eye of Listing,

or even from the eyes of the small number of persons whose visual function has been minutely tested, to the properties, as optical instruments, of the eyes of mankind in general. "La position," writes Helmholtz, "des foyers, des points principaux et des points nodaux de l'œil est assurément soumise à des variations individuelles assez importantes, puisque la plupart des mensurations de l'œil et de ses diverses surfaces réfringentes présentent, chez différents sujets, des différences plus grandes qu'on ne paraissait devoir les attendre pour un organ dont les fonctions semblent réclamer une si grande exactitude de construction."

As a matter of fact, the theoretical limit of resolving power assigned by Lord Rayleigh, to which he tells us that civilised physicists have "approached," is one which civilised physicists have considerably exceeded. The mean of twelve observers, as quoted by Helmholtz, gives resolving power under a visual angle of 101 seconds; and this mean is reduced by two cases in which the angles were 124 and 147 seconds respectively. The minimum was 51 seconds, the most frequent angle was about 80 or 90 seconds. The commonly accepted standard of normal vision among civilised people is satisfied by deciphering letters the parts of which subtend visual angles of one minute, while each letter as a whole subtends a visual angle of five minutes.

I cannot say, however, that I think any such tests are very material to the issue. The eyes of civilised physicists, or of such of them as have undertaken practical research in physiological optics, are probably very highly cultivated, and I doubt whether resolving power, which must depend chiefly upon the functional activity of the central depression of the retina or, in the case of stars, upon the functional activity of the zone which immediately surrounds the yellow spot, furnishes any accurate test of acuteness of vision in the sense in which I employed the phrase.

Assuming the civilised man and the savage to have eyes of precisely equal optical value, the latter might yet possess an acuteness of vision greatly in excess of that of the former; and this excess might be due to conditions of the percipient elements of the retina which, in the case of the savage, permitted the optical powers to be utilised to the fullest extent. The savage might have greater sensitiveness to variations of light, greater sensitiveness to colour, and acuteness of vision over a larger retinal area. All these advantages might be conferred by better formation or higher development of the retina, and such higher development might at once be promoted by exercise and handed down by descent. I support the "commonly-received view" that the vision of savages is more acute than that of civilised men, because this view seems to me to be established by abundant testimony, and to be in perfect harmony with physiological knowledge. I feel very strongly that the conditions of town life are unfavourable to the evolution of the eye and favourable to its involution or degradation; and I believe that a moderate amount of attention might greatly modify these conditions, and might do for the eyes what is done by athletic games and exercises for the muscles.

With regard to the improvement of Lord Rayleigh's own vision, in a dim light only, by concave glasses, I think his Lordship cannot fail to see that the case, as stated, does not contain all the data which would be required in order to arrive at an explanation of the phenomenon.

R. BRUDENELL CARTER

IN a short article on Civilisation and Eyesight which appeared in NATURE of February 12, Lord Rayleigh expresses the belief that the greater visual acuity of savages "is a question of attention and practice in the interpretation of minute indications" and is not ascribable to any possible inherent superiority in their eyes, regarded simply as optical instruments. With this conclusion probably most who have had opportunities of testing the sight of uncivilised races or read the account given by those who have undertaken such examinations, will agree. The same difference in making more or less out of an imperfect retinal image is met with in different individuals with the same degree of short sight, and otherwise subjected to similar conditions according as they have or have not been in the habit of resorting to constant optical correction of their defect. Such a cerebral elaboration of the retinal image, as it might be called, constitutes also probably the main reason for the difference between the visual acuity of children who have only just learnt to read the letters of the alphabet and adults, which our ordinary tests so frequently show.

The question of the increasing prevalence of short sight has for a considerable time been the subject of much investigation and speculation in Germany, the results of which have been in many cases to give rise to predictions of rather an alarmist tendency. These, again, have led to legislation in the shape of regulations with respect to school appliances which might meet the theoretical requirements of the most energetic and influential agitators. It is to be hoped that, as the question is now being brought forward in this country, it will be viewed in a more comprehensive manner. The numerous statistics from German schools have shown that the proportion of short-sighted boys continually increases from form to form, and from this fact it is very generally argued that the continued use of the eyes for the perception of near objects is the essential if not the only factor in the production of short sight. This view appears, again, to be supported by statistics which allot the largest proportion of short-sighted individuals to those branches of industry or those pursuits which constantly call for near vision. Two points, however, appear to be forgotten, or at all events fail to receive sufficient consideration, in arriving at such a conclusion. In the first place, there is an undoubted tendency to increase in the degree of short sight with age alone up to the period of cessation of growth. This has been shown to be due to the elongation of the antero-posterior axis of the eye, which carries the retina further and further from the principal focus of the dioptric media, and is in the vast majority of cases no more a disease than is the attainment of a greater than average height by a certain number of individuals. It is merely a type, and as such is governed by the laws of heredity. A small proportion of cases of short sight are, however, due to disease. These differ from the ordinary cases in that they are seldom hereditary and are not more frequently present in the learned than in the absolutely illiterate classes, besides which the pathological changes to which they are due can often be detected with the ophthalmoscope. The second point which has to be taken into consideration is how far the greater proportion of short sight amongst literary men, or artisans whose daily work necessitates close vision, is actually due to their occupation, or depends on the circumstance that, being originally short-sighted, they have drifted into pursuits which are more attractive to them, owing to their not being able to enjoy out-door work or sports to the same extent as others whose eyes are more fortunately focussed. That the choice of a life-occupation is often influenced by the condition of the sight is a matter of every-day experience, and it would be interesting to have statistics showing to what extent this occurs in the case of myopia. Further, as a man's circle of acquaintance is, for the most part, amongst individuals having similar interests in life, intermarriage in myopic families must frequently occur, and would tend to perpetuate, and perhaps increase, the defect. In savages, on the other hand, where the great principle of the survival of the fittest is not frustrated to the same extent as among civilised races, everything would evidently be against the perpetuation of the myopic type. The question comes to be, then, Is not the absence, or comparatively great infrequency of short sight amongst savages due rather to the requirements of such races being antagonistic to the circumstances which would be most likely to perpetuate the myopic type, than to the fact that young savages are not subjected to compulsory education? The pages of NATURE are perhaps hardly the place to develop very fully a question of this kind; suffice it to say, therefore, that the conclusion which such reflections, as well as the result of every-day examination of cases of short sight, appear to justify, is, that *the increase of myopia is due mainly to the perpetuation of a type through the requirements of civilisation*, and, though not a disease in the ordinary sense, it is desirable to attempt to check its progress. This will assuredly not be an easy matter, but it is not likely to be much influenced by such school reforms as have been introduced into Germany.

Lord Rayleigh mentions, as an interesting subject for further investigation, the slight myopia which he finds not uncommon when the light is lowered in a room, until objects begin to be indistinctly seen. He finds, *e.g.* that though in a good light he sees rather worse with a concave lens of 36 inches focus than without it, yet, when the illumination is diminished, the same lens increases his visual acuity. Altogether, the influence of illumination on visual acuity, and the relation between light-sense and form-sense, are points which have not yet received adequate attention. If the phenomenon described by Lord Rayleigh be really one of short sight occurring under the circumstances mentioned, it is evident that it can only be due to involuntary accommoda-

tion for a nearer point than that on which attention is directed—a kind of spasmodic myopia, and, as such, would disappear when the power of accommodation was paralysed by atropine. On the other hand, it may not be myopia at all, the improvement given by the weak concave lens being perhaps due to the contraction of the pupil, which would occur along with the accommodation necessary to neutralise the effect of the glass. If this were the case, the improvement would also take place by the use of a suitable diaphragm held in front of the eye. Still another possible explanation suggests itself, viz. that the new dioptric combination made up of the concave lens and partially accommodated crystalline might introduce conditions of chromatic and spherical aberration which were more favourable to distinct vision. The disturbing effects of such aberration are probably greatly neutralised by the arrangement of the retinal elements, but the degree of the neutralisation is, not unlikely, dependent on the amount of absolute and relative illumination of contiguous elements. GEO. A. BERRY

Edinburgh

The Fall of Autumnal Foliage

THE paper by Mr. Sorby in NATURE for December 4, 1884 (p. 105) opens up an unpursued inquiry into the cause of leaves falling in autumn. While Mr. Sorby has had his attention drawn to the subject by looking at the actual trees and leaves "of the fine display of autumnal tints which we have lately seen" in England, there is much of both positive and negative evidence to be drawn in two extreme directions—the tropics and the pole.

Being, in the year 1881, home from India, where, it is not necessary to say, nearly all the trees retain their green foliage throughout the year, the writer indulged in a long curiosity to see the counties of Caithness, Orkney, and Shetland He went there with reference to the luminosity, which reaches its maximum in them for Great Britain, and is very marked and exceedingly striking and beautiful as a feature all over the north of Scotland in the month of June, when it is daylight all through the hours of night, sufficiently clear for reading distinct print at twelve o'clock midnight.

A peculiarity of Caithness and the Orkney and Shetland Islands is that no forest-trees can be got to grow. Setting on one side a remark "that it was because nobody had tried," the suspicion had already occurred to my mind that there must exist some other causes than those usually asserted—the high sea winds, bleakness in winter, and extreme cold—for this want of trees.

Any one who has been much in the north of Scotland, and is at all acquainted with the optical sciences, cannot fail to have noticed the immense amount of polarised light there is from the sky; almost all the diffused daylight, except for an hour or two in the middle of the day, being plane or elliptically polarised.

The attention of readers of NATURE may with advantage be specially directed to the possibility, from the phenomena of the north, that leaves fall in autumn from trees growing above a certain latitude—about 30°—through loss of vitality in the more or less highly polarised light.

The first thing a traveller from India notices in Alexandria is the American fall of the leaves in the Grande Place, or, as a fellow-passenger once put it, pointing to these, "It is here trees first become deciduous" It is worth being remarked that, not until reaching Cairo or Alexandria, can sun-protection be done without.

So far Mr. Sorby has to refer to the action of light in the last resort, as he says, with regard to leaves, "slight frosts reduce their vitality in such a manner, that the chlorophyll is changed by the action of the light into a red product."

Chlorophyll is composed of carbon, hydrogen, oxygen, and a trace of iron. Chemically it is $C_{18}H_{20}N_2O_3 + O_{18}$, resulting from the action of carbonic acid and ammonia on a fat, $C_8H_{14}O$, under the influence of light, as given by a different authority; but the composition of its products and combinations have not been traced. Still there is almost every constituent of the animal frame present except the earthy salts, and it must be a substance very sensitive to rays of light, or to what light probably is, electro-magnetic forces.

The weakening of the plant is supposed by Mr. Sorby to have occurred, for the leaves of a tree to have lost the vitality which counteracted the chemical degradation of the chlorophyll. Now in India or Ceylon, if a stalk were injured, the leaves would wither into brown. Trees remain, however, when living, constantly green, the leaves dropping off gradually one by one almost, and are immediately replaced. Indian leaves of trees are much thicker, and more of the texture of parchment than those of foliage in European countries, and the phenomena of change can be studied in evergreens without going there, Indian observation merely serving to draw attention that might not otherwise be given to the matter.

The Rothamsted experiments of Sir J. B. Lawes and Dr. Gilbert, F.R.S., bear closely on the question. They found (Swansea, 1880, address) that plants assimilate chlorophyll not only during but a small portion of the year, but the action is limited to the hours of daylight, while during darkness there is rather loss than gain. The experiments, however, both there and in Norway by Prof. Schübeler, were made in ordinary unpolarised solar or electric light.

On the other hand, in India the light is intense owing to its tropical position, and, from the altitude of the course of the sun, very slightly polarised. It is only for an hour at dawn and another hour of sunsetting that the Indian is at all the same sort of daylight that it is in England. It accords with the Rothamsted and Norwegian experiments under the continuous exposure of vegetation to daylight and electric illumination during the night that the trees in India are large and evergreen. Of course in time leaves have done their work and fade, but as they have not been unfolded simultaneously, they drop off gradually in batches.

Where, accordingly, the light is polarised, trees are scarce or absent, mown by a swathing light; and in the tropics, where there is little polarisation, they are luxuriant, and green all the year round.

This is not inconsistent with fact. To begin with, plane polarised light has half the intensity of ordinary white light, the set of vibrations at right angles to the plane of polarisation being absorbed in the reflecting matter of the sky. Besides, circularly or elliptically polarised light must largely prevail, to judge from the metallic glow there is on the Pentland Firth, Orkney, and Shetland in midsummer, and what effect circularly polarised light has on the assimilation of carbon in the leaves of plants and decomposition of chlorophyll is unknown.

At any rate, Caithness, and the northern islands have a number of hours in the daytime of a wintry darkness, and scarcely any light in the summer months and its long days that is not polarised. From this cause, which could in the leisure of their winter be put in arithmetical units of force, combined with cold winds and a thin soil, without alluvial deposits, resting on stone, it is no wonder that, though the inhabitants are not strangers to the pathos of the fall of the leaf, the Caithness-shire landscape, and the sward and heather of Orkney and Shetland are lustrous day and night with polarised light, and bare of autumnal foliage. A. T. FRASER

India, January 22

Erosion of Glass

IN reference to the letter of Dr. Ord in last week's NATURE, glass is by no means proof against the action of either acids or alkalies, indeed its resisting seems to depend merely on its colloidal, at any rate non-permeable, nature. It may not be generally known that water alone very rapidly acts on glass, especially when it is in a finely divided state, extracting both alkalies and silica in quantity. It would be rash to put down the action of substances on glass to "molecular coalescence" to the exclusion of chemical action, or under the idea that acids or fluorine are necessary to etch glass. Alkaline salts, especially phosphates, act, either wet or dry, very vigorously on glass. One class of salts, the potassium salts of phenol sulphonic acids, have been noticed to literally tear a glass bottle in pieces, whilst crystallising out of an acid solution. Ordinary gum is often acid in reaction; but the ordinary mechanical action of sticking and then contracting is probably quite sufficient to cause an abrasion or etching, especially with soda-glass. This purely mechanical action is often noticed in the distillation of tarry substances which solidify at a high temperature, the whole interior surface of the retort being torn off and cracked in all directions. W. R. H.

A Lantern Screen

THE optical lantern has come to be so much used for scienti fi and educational purposes, that you may perhaps think it use fu

to your readers that a screen, whose valuable properties seem even now to be scarcely at all known, should be noted in your columns.

It simply consists of a sheet of French tracing-paper, of a kind which possesses a remarkably dull, non-reflecting surface. With this screen and only an oil-lamp lantern, it is quite easy to show pictures well to a couple of hundred people in a room fairly well lighted—sufficiently lighted indeed to enable note-taking or reference to books to be accomplished with perfect ease—provided that extraneous lights are not placed *behind* the screen.

It was to Mr. George Smith, of the Sciopticon Company, that I was indebted, four years ago, for the knowledge of this fact; which, with considerable lantern experience, I scarcely knew how to believe, until I had myself verified it.

At present, however, the tracing-paper cannot, I believe, be obtained more than three to four feet square.

CHARLES J. TAYLOR

Toppesfield Rectory, Essex, February 17

Fuller's Earth as a Filter

WHERE the *fuller's earth* is dug from the Bedfordshire greensand it is held in much repute for its efficacy in removing impurities from turbid water.[1] In addition to the other uses to which it is here applied, dealers take it around through the fen countries, and dispose of it for clarifying the peaty water,[2] often the only supply obtainable in those districts.

I shall esteem it a favour on the part of the readers of NATURE residing on the Greensand or Oolites of the southern counties to notify if these filtering properties of the Bedfordshire fuller's earth are in any way unique—in so far as they appear withheld from that of other places?—as at Reigate, Bath, &c., where fuller's earth is known to them to be dug.

Bedford, February 23 A. G. CAMERON

The Boomerang in India

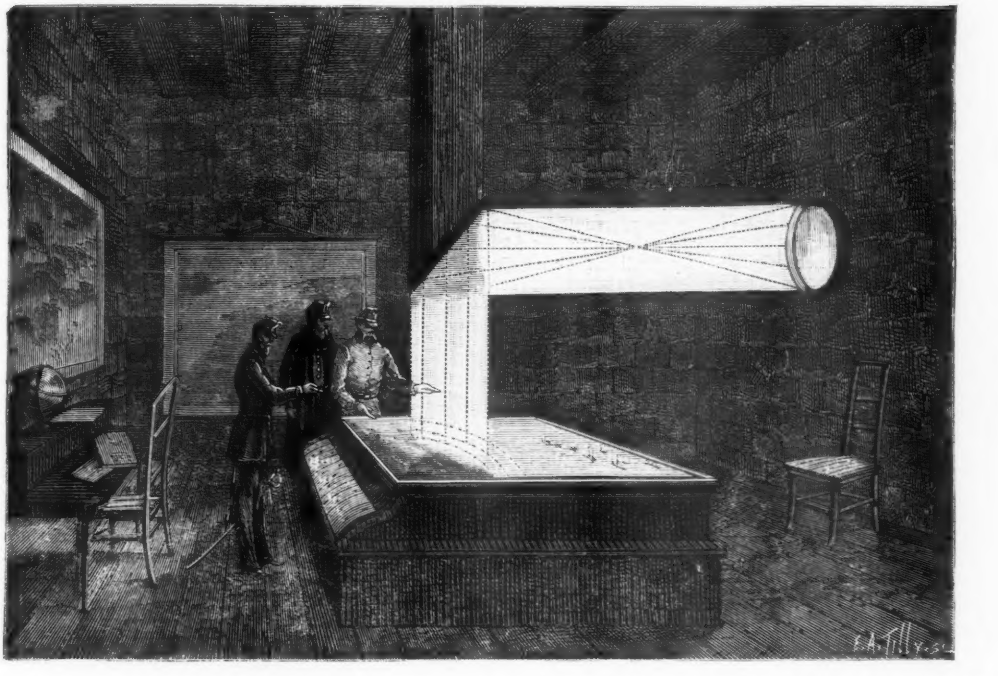
IN Gustav Oppert's work "On the Weapons, Army Organisation, and Political Maxims of the Ancient Hindus," the boomerang is mentioned as being among the weapons, especially in Southern India, and made of various materials—iron, ivory, and wood. Are any specimens to be found in our museums here, or would any private persons who may happen to possess any, kindly allow me to inspect them?

ARTHUR NICOLS

THE CAMERA OBSCURA IN TORPEDO WORK[1]

AT the time of the last Austro-Italian war, in 1866, the Austrian Government made the greatest efforts to protect its harbours from an attack of the Italian fleet. Torpedoes were placed in great numbers in the harbours, and the greatest vigilance was enjoined on all the commandants of such places.

The accompanying illustration represents a novel application of the camera for use at the observing or firing post of a party belonging to the military telegraph. The torpedoes are placed along certain concentric lines, very close to each other, and at a certain depth below the sur-



face of the water, no sign of their presence being apparent. A metallic wire connects each of them with a post of observation situated on the coast at a point sufficiently elevated to permit of the port being seen. According to well-known optical laws, an image of the port is formed on the glass. Black points marked on that image indicates the exact position of each torpedo; these points are all numbered, each number corresponding with that on a particular key of a keyboard. To press one of these keys with the finger is sufficient to place the corresponding

[1] From *La Nature*.

[1] *Geol. Mag.*, February, 1885.

[2] A brief account of the meth d in use in the fen districts of Cambridgeshire and Lincoln will appear shortly.

torpedo in communication with an electric battery, by means of a metallic wire which connects it with the port, and so cause an explosion.

THE CONTINUITY OF THE PROTOPLASM IN PLANT TISSUE

The translation of Dr. Schaarschmidt's paper in a recent number of NATURE (January 29th, 1885) gave those who, like myself, are unable to read Hungarian, full details both of his researches and views, and as one much interested in the subject of "the continuity of the protoplasm," I should like to be allowed to make a few remarks upon it.

To refer first of all to a matter of minor importance with reference to sieve-tubes, I see that Dr. Schaarschmidt says: "The first direct observation was made in 1854 by Hartig, and not by Sachs, as Walter Gardiner states." This refers to the opening passage of my paper in the *Arbeiten des Botanischen Instituts in Würzburg*, Band III., Heft I., where I say: "A most important addition was made to our knowledge of the histology of tissues in 1863 by Sachs, and in the following year by Hanstein, when they demonstrated that in the sieve-tubes first described by Hartig there are perforations in the transverse walls, &c." I made that statement, relying, as I still do, on the authority of Prof. Sachs's text-book (English edition, 1882, p. 89), since it seemed to me that Hartig's observation, which could not be confirmed by "Mohl and others," was actually proved and demonstrated beyond doubt by Sachs and Hanstein, and, moreover, in fresh and not in macerated tissue.

With respect to the main subject under immediate consideration, I shall first make one or two general statements as to the continuity of the protoplasm in plant tissues. In my paper in the *Würzburg Arbeiten*, to which I have already referred, I have spoken of two appearances of continuity: one which I speak of as direct, and the other as indirect. By direct or unbroken continuity, I mean the appearance of a thick protoplasmic process, extending between, and uniting the protoplasmic contents of two contiguous cells: the pits forming one continuous canal, and being uninterrupted by a pit-closing membrane. In this case, therefore, the idea of open pits is necessitated. By indirect continuity I mean the existence of a pit-closing membrane between the two opposite protoplasmic processes in the pits: the membrane being perforated in a sieve-like manner, and thus allowing the two protoplasmic processes to become united to one another by means of delicate protoplasmic filaments which traverse the pit-closing membrane. I further stated that my observations led me to believe that a pit-closing membrane was present in all cases, and that the appearance of a direct unbroken continuity is fallacious. (See also *Roy. Soc. Proc.*, December 13th, 1883.)

Turning now to the consideration of the observations made upon the Florideae, I shall have to differ somewhat with Dr. Schaarschmidt; but, while I do so, I wish it to be quite clearly understood that I do not in the least undervalue the work of those investigators to whom I refer, and who, according to my view, have not actually demonstrated a continuity of the protoplasm from cell to cell, but have only observed facts which render the existence of such a continuity extremely probable. Thus, since I regard the perforation of the pit-closing membrane as proving continuity, I hold that the observations of Bornet, Perceval Wright, and Agardh (I have unfortunately not seen Kolderup-Rosenvinge's paper) have not demonstrated continuity, but have demonstrated that the pit-protoplasm clings with remarkable tenacity to the pit-closing membrane. Hick has simply repeated the observations of these investigators, and of his results the same may be said. Since, therefore, Schmitz (1883) has found that a pit-closing membrane does exist; that it is perforated in a sieve-like manner, and that therefore the continuity is not direct, but indirect, it seems to me that to him alone belongs the credit of having demonstrated the continuity of the protoplasm in the Florideae, and I have myself (*Proc. Camb. Phil. Soc.*, February 11th, 1884) been able to confirm his results as to the existence of the closing membrane in question.

In considering the history of the subject, and leaving sieve-tubes out of the question, it is clear that Tangl's observation (1880) on the endosperm cells of *Phœnix* and *Strychnos* was the first new discovery in the direction of the continuity of the protoplasm between neighbouring cells. Then came Strasburger's classic work on the cell-wall ("Bau und Wachsthum der Zellhäute," 1882); his observations on the porosity of the pit-closing membranes, and his valuable suggestions as to the probability of cell-wall perforation, together with the citation of instances which already occurred, and his extremely interesting observation with regard to the swarm-spores of *Vaucheria*. Naturally *Volvox*, *Pandorina*, and the zoospores of *Hæmatococcus* offer other examples of the perforation of the cell-membrane by protoplasm.

After Strasburger came Russow. Russow read his first paper at the January meeting of the Dorpat Society (*Sitzber. d. Dorpater Nat. Gesell.*, 1882), but it did not come into my hands until some time after I had published my first observation (*Quart. Jour. Mic. Sci.*, October, 1882), so that, at least from that point of view, my work was quite independent and original. As to the order of the other papers, I agree with Dr. Schaarschmidt, except that I would like to add to his list the papers of Pfurtscheller (*Selbstverlag des k. k. Franz Joseph Gymnasiums*, 1883), Will, (*Bot. Zeit.*, 52, 1884), Tangl (*Sitzb. der k. Akad. der Wiss.*, Bd. 90, 1884), and Goroschankin (*Bot. Zeit.*, 41, 1883).

As to Dr. Schaarschmidt's claiming, in 1884, the suggestion of the universality of the occurrence of continuity of the protoplasm in plant-cells, I think that, considering the state of the subject at that time (April, 1883) something may also be said in my favour, for I find in my Royal Society paper (*Phil. Trans. Roy. Soc.*, April, 1883) the following statement:—"Although I am aware of the danger of rushing to conclusions, I cannot but remark that when these results (which were foreshadowed by Sachs and Hanstein, when they discovered the perforation of the sieve-plate) are taken in connection with those of Russow, it appears extremely probable that not only in the parenchymatous cells of pulvini, in phloem parenchyma, in endosperm cells, and in prosenchymatous bast fibres, is continuity established from cell to cell, but that the phenomenon is one of much wider, if not of universal, occurrence."[1]

Passing on to the results of Dr. Schaarschmidt's second paper, to which he refers, where he gives a very long list of tissues in which he has demonstrated the existence of a continuity of the protoplasm, I should only wish to remark that while he appears to have observed in a satisfactory manner, and with comparative ease, cases that have appeared to me to be excessively difficult, yet his figures of such continuity are not satisfactory, and in many of them it is the direct and not the indirect continuity which his drawings represent. As I have stated elsewhere (*Arb. d. Bot. Inst. Würzburg*) an examination of fresh unswollen tissue with iodine and chlor. zinc. iod. will always demonstrate the presence of a pit-closing membrane.

I now come to a subject which I approach with some regret, since, in dealing with it, I have to dissent from the expressed opinions of a number of competent observers, and especially do I feel this regret with regard to one of those papers—viz. that by one of the most distinguished

[1] Mr. Dyer has already very kindly alluded to this subject on my behalf. It will be observed from the text that at that time (April, 1883), owing to the hurry of publication I had not referred to Hartig's paper (February 18, 1885).

investigators of plant histology: Prof. Russow. The subject is that of the existence of intercellular protoplasm.

Dr. Schaarschmidt has already given the literature. The only other observation with which I am acquainted is that of Prof. Frommann ("Zur Lehre von der Bildung der Membran von Pflanzenzellen"—separate pamphlet) who finds in the intercellular spaces of the young stem of *Ricinus communis*, protoplasm; starch grains, and chlorophyll grains.

The observations as to the existence of intercellular protoplasm depend chiefly upon the staining reactions of iodine and sulphuric acid or chlor. zinc. iod. The cell wall turns blue, or remains yellow as the case may be: the protoplasm, and in certain cases a substance; in or lining the intercellular spaces, stains dark brown. Or again in some instances—*e.g.*, the rhizome of *Aspidium filix mas*—the substance in the intercellular space remains uncoloured. Other observers have employed other staining reagents after swelling with sulphuric acid and chlor. zinc. iod.—*e.g.*, saffranin, eosin, or anilin blue—and in this case a colouration is observed of the protoplasm on the one hand and of the intercellular space substance on the other.

Dealing first with the iodine and sulphuric acid or chlor. zinc. iod. method, it is obvious that, besides the protoplasm which assumes the well-known dark brown colouration, any lignified, cuticularised or suberised membranes would react in the same way, and in the case of one of Berthold's (*Ber. d. Deut. Bot. Gesell.*) examples, *e.g.*, young stem of *Ligustrum vulgare*, the substance which so markedly stains, does actually consist of the external membrane of the intercellular space, which towards the free surface has undergone changes associated with partial lignification (Gardiner, *Proc. Camb. Phil. Soc.*, Nov. 10th, 1884), as can be readily proved by treating a section with aniline chloride and hydrochloric acid, when the well-known gold yellow reaction of lignified tissue appears. In the same way the substance which does not stain with iodine and sulphuric acid might be of a mucilaginous nature, and like the mucilage of the external portions of the wall of the seed of *Ceratonia siliqua* give the same reaction, viz., remain uncoloured. But there are cases which are not so easy to deal with, as I have stated elsewhere (*Proc. Camb. Phil. Soc.*, Feb. 11th, 1884), I found that the Hofmann's blue which I had so successfully employed for demonstrating the existence of protoplasmic filaments in the pit-closing-membrane stained not only protoplasm but also certain forms of mucilage. Like Russow (*Sitzber. d. Dorpat. Naturfor.*, Sept., 1883), I thought at first that in *Aucuba Japonica* I had discovered the existence of intercellular protoplasm, but I observed later on that this staining substance could be seen to arise as drops on the external walls and that these drops went blue with iodine: thus demonstrating that they were not protoplasm but mucilage. I therefore made experiments with the methylene blue which I had found (*Phil. Trans.*, part iii., 1883) to be so useful as a stain for the cell wall, and so differentiating in its action. (A solution is made in water containing a trace of alcohol; the solution being diluted with water before use. The section freed from alcohol by repeated washing, is left to stain for about 20 seconds, washed and mounted in water). I further found that methylene blue stains equally well, all substances formed by the degeneration of cellulose walls, such as mucilage and the like. So while Hofmann's blue stains protoplasm and mucilage, but not cell wall, methylene blue stains cell-wall and mucilage but not protoplasm. Thus the cell-wall and protoplasm may be readily discriminated in a very satisfactory manner, and without this reaction it would indeed be hard to distinguish the two. Many dyes behave like Hofmann's blue so far as the staining of the mucilage is concerned, and I have little doubt but that eosin resembles it in this respect, though not such a good differentiating stain for the protoplasm. In the course of all my experiments, which I have repeated several times, I have never found intercellular protoplasm but often intercellular mucilage. In all cortex tissues which are often remarkable for their mucilaginous character —*e.g.*, *Viscum*, *Fraxinus*, *Ilex*—mucilaginous degeneration of the free cell-walls very usually occurs, which often—*e.g.*, *Ilex*, *Viscum*—extends even to the whole middle lamella. In *Aspidium filix-mas*, *Blechnum Braziliense* and other ferns, theso-called cuticularised threads (cuticularfäden) are in reality rods consisting mainly of mucilage which arise as drops on the free surface of the cell-wall and increase in length by repeated basipetal formation. I do not therefore find myself able to allow of the existence of intercellular protoplasm.

As to the middle lamella being protoplasm I can only refer to the statements I made with regard to Frommann and Elsberg's researches (*Quart. Jour. Mic. Sci.*, March, 1883) and I share fully in the opinion of Prof. Russow ("Ueber die Auskleidung der Intercellularen," *Sitzber. d. Dorpat. Naturfor.*, August, 1884) that if such were the case it is clear that we could have no such thing as a mass of tissue resisting great stress. The cells cannot be connected together by protoplasm. As to the existence of the intercellular chlorophyll grains of which Dr. Schaarschmidt speaks, and the chlorophyll grains and starch grains observed by Prof. Frommann, I also share Prof. Russow's view (*loc. cit.*) that the above investigators must have been deceived by some abnormal appearance, for what could be the physiological significance of such a phenomenon? The full details of my researches on the subject will, I hope, shortly appear in the *Quarterly Journal of Microscopical Science*.

WALTER GARDINER

Botanical Laboratory, Cambridge, February 10

THE BANGOR LABORATORIES

THE following is a description of the Laboratories of University College, Bangor, which were opened by Sir William Thomson on the 2nd inst. The illustration shows the ground floor arrangement; in the upper floor are a magnetism-room and an optical gallery.

The new physical and chemical laboratories occupy the site of the old stables and coach-houses of the "Penrhyn Arms Hotel," which is now used as the main building of the College; and, to lessen expense, a plan has been adopted by which the old walls are, as far as possible, taken advantage of for outside walls and partitions. To utilise the available space to the utmost it was decided to roof in the whole area, which measures about 120 feet by 80 feet. This area is bounded on the east by the main building of the College; on the south by a private road which runs nearly parallel to the Shrewsbury and Holyhead turnpike road, and gradually ascends until opposite the laboratories, the ground is about 20 feet above the level of the turnpike; on the west and north by the private grounds of the College.

At the extreme east end of the south front of the laboratory buildings is a wide door opening into a vestibule, from which a passage leads north, and terminates in a wider space or hall. From this hall a long corridor runs parallel to the south front, dividing the floor space into two nearly equal parts. Of these the southern is set apart for physics, the northern for chemistry.

The physical and chemical lecture-theatres (23, 41) are of the same size, 32 feet square and 19 feet high, and are placed side by side with the corridor as a separating space between them. The internal arrangements are nearly the same in both rooms. The students' entrances are opposite one another in the corridor. The benches, eight in number, rise from the front to the back of the room, and front toward the west. The lecture-tables are placed about 4 feet from the front bench, and between each table and the west wall there is a clear space between 7 and 8 feet wide. In the physical lecture-theatre the table is supported on four pillars of masonry, and is en-

tirely independent of the floor of the room. The tables differ somewhat in their arrangements, one being designed specially for chemical, the other for physical, work. Both have been fitted with the most recent improvements, and some novel appliances.

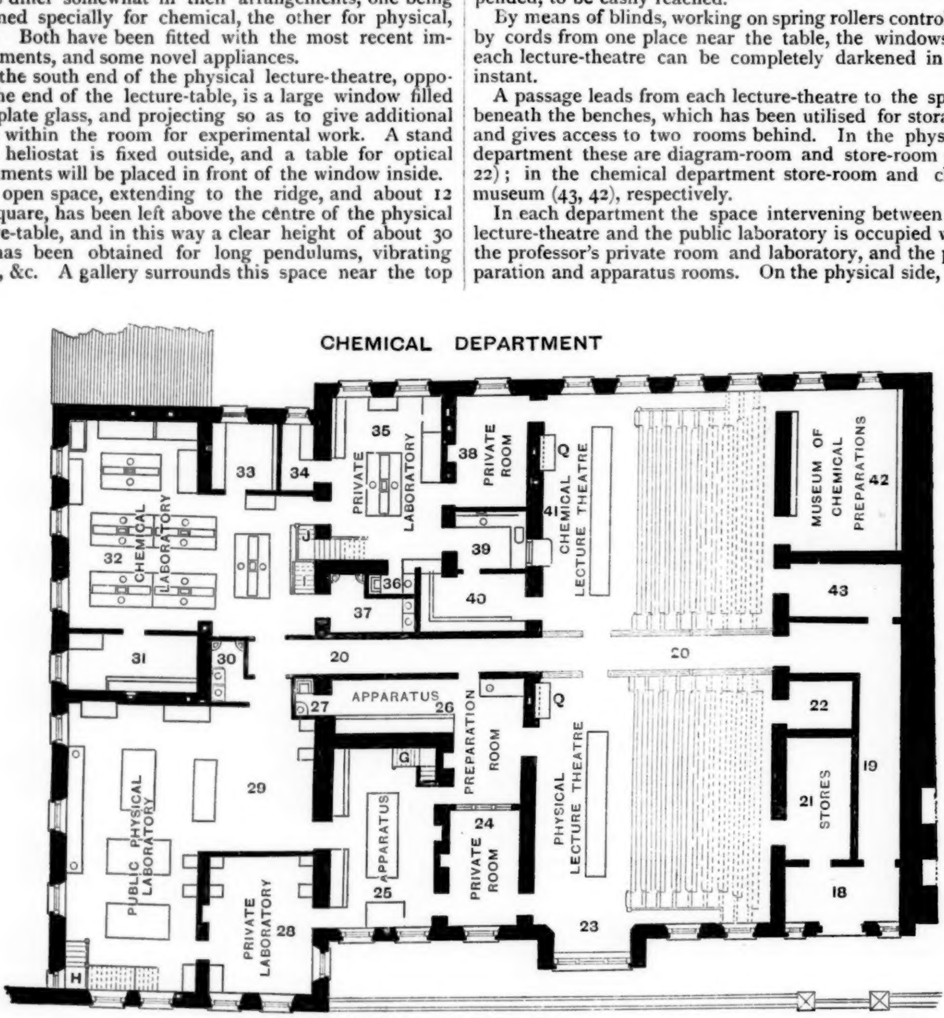
At the south end of the physical lecture-theatre, opposite the end of the lecture-table, is a large window filled with plate glass, and projecting so as to give additional space within the room for experimental work. A stand for a heliostat is fixed outside, and a table for optical instruments will be placed in front of the window inside.

An open space, extending to the ridge, and about 12 feet square, has been left above the centre of the physical lecture-table, and in this way a clear height of about 30 feet has been obtained for long pendulums, vibrating cords, &c. A gallery surrounds this space near the top to allow the beams, from which such appliances are suspended, to be easily reached.

By means of blinds, working on spring rollers controlled by cords from one place near the table, the windows of each lecture-theatre can be completely darkened in an instant.

A passage leads from each lecture-theatre to the space beneath the benches, which has been utilised for storage, and gives access to two rooms behind. In the physical department these are diagram-room and store-room (21, 22); in the chemical department store-room and class museum (43, 42), respectively.

In each department the space intervening between the lecture-theatre and the public laboratory is occupied with the professor's private room and laboratory, and the preparation and apparatus rooms. On the physical side, immediately to the west of the lecture-theatre, are the private room and preparation room (24, 26), which communicate directly with the lecture-theatre. Beyond the private room is the apparatus room (25), which overlaps the preparation room, and communicates directly with it. The preparation room is L-shaped, one limb of the L running back along the north wall of the apparatus room. This part of the room is open to the roof, to allow the room to be lighted by a window at the west end, placed above the level of the western part of the building, and by a large skylight, and is fitted along one side with cases as a supplementary apparatus room. An available height of about 25 feet is obtained in front of these cases, and is designed for elasticity experiments. A gallery over the window allows the roof above this part of the room to be reached by means of a ladder. The limb of the L immediately behind the lecture room is fitted with a work-table and provided with a fireplace, as a preparation room for the lecture experiments.



The New Laboratories of University College, Bangor.

To the west of the apparatus room is the private laboratory (28), about 20 feet by 18 feet, communicating with the preparation room and private room on one side, and with the public laboratory (29) on the other. In the south-west corner of this room a slate balance table has been built into the wall, and near the other end of the room slate tables have been fixed to the wall for galvanometers and electrometers. A single work-table, fitted with gas, occupies the centre of the room, and a smaller work-table with gas and water the south-east corner.

The general physical laboratory (29) is a large room over 1000 feet square in area, running along the west end of the space allotted to the physical department. The

student's entrance is from the corridor, and adjoining this entrance is a lavatory and cloak-room (30) for their use. The laboratory is fitted with work-tables for about twenty students. Along the west wall is a long work-table, fitted with gas and water. The space below this table is divided into cupboards and drawers for the use of students. On the north wall a draught chamber, and combined heating store and sand bath, of novel design, have been provided, and at different positions slate slabs for galvanometers, &c., have been fixed to the walls as in the private laboratory. The roof is left open to add to the height of the room, and for convenience for experimental arrangements. The room is lighted by windows in two sides, and by a row of large skylights on each side of the ridge.

A staircase, G, leads from the apparatus room to two rooms on the second floor. One of these has been constructed without iron to ensure a uniform magnetic field. No magnets or a large mass of iron will be stored below, and the room will be available for absolute electric measurements. The other room is a gallery about 37 feet long and 10 feet wide, constructed for optical and photometric work.

A second stairway leads to a small room communicating with the gallery above the lecture table, and with a flat space on the roof which has been constructed as a station for observations of atmospheric electricity. The collector of electricity will be placed outside on the flat roof, and connected with a station electrometer in the small room below.

A stair, H, at the south end of the general physical laboratory leads to some valuable rooms in the basement, which have been set apart for practical electricity, workshop (with lathe and forge), magnetic room, battery room, store room, &c.

Returning to the chemical side, the preparation and apparatus rooms (39, 40) occupy the position corresponding to that of the preparation and elasticity rooms on the physical side. The preparation room is fitted with proper work-tables, and communicates with the lecture-theatre by a sliding panel. The private laboratory (35) corresponds to the apparatus room of the physical department. It is fitted with a work-table for four persons, and a large draught chamber and sand bath, and communicates with a special balance room (34) on the west side. The general chemical laboratory (32) corresponds in position with the physical laboratory, and is fitted with work-tables for twenty-four students. These tables have been constructed according to a special design embracing all the most recent improvements. Around the north end of the laboratory have been placed sand baths, draught chamber, large distilling table, sink and table for water and air baths. A portion of the south end of the laboratory has been partitioned off and fitted up as a combustion and blowpipe room (31). At the north end a balance room (33) has been fitted up, and in this room, as well as in the private balance room, the floor is completely isolated from the laboratories, and the tables for the balances are supported on strong brackets firmly fixed to the stone walls. The lighting of the laboratory is managed in the same way as that of the physical laboratory, the skylights along the east side of the ridge of the roof being made to open.

A staircase, J, leads from the general laboratory to the first floor of the chemical department, which is occupied with rooms specially designed and fitted for photographic, gas analysis, and spectroscopic work respectively. A ladder leads to a flat roof for experiments which require to be made in the open air. A second stair, I, leads from the general laboratory to the basement, where there is a rough operation room, joiners' shop, and metallurgical room.

The arrangement of the rooms and the construction of the lecture tables, work-tables, and other fittings have been carried out by Mr. Richard Davies, architect, Bangor, under the direction and superintendence of Profs. A. Gray (Physics) and J. J. Dobbie (Chemistry), and in accordance with sketch plans furnished by them.

The addresses on Scientific Laboratories which Sir William Thomson delivered on the opening of the above laboratories we shall give in our next number.

NOTES

THE Geological Society has this year awarded the Wollaston Medal to Mr. George Busk for his researches on Fossil Polyzoa and on Pleistocene mammalia; the Murchison Medal to Prof. Ferdinand Roemer, the eminent palæontologist of Breslau; the Lyell Medal to Prof. H. G. Seeley, for his long-continued work on Fossil Saurians; and the Bigsby Medal to M. Renard, of the Brussels Museum, on account of his petrographical researches.

THE annual meeting of the Paris Academy of Sciences was held on February 23 before a very large audience. M. Rolland, the President for 1884, was as usual in the chair. He delivered the customary address, alluding to the members of the Academy who died during the past year, and gave a *résumé* of the principal scientific facts of the same period. M. Arago (Emmanuel), the eldest son of François Arago, who is French Ambassador to Berne, had come to Paris in order to be present at the delivery of the *éloge* on his illustrious father, who died thirty years ago. The delay must be attributed to the political career of the Perpetual Secretary of the Academy of Sciences, who, having been a member of the Government of the second French Republic, was not a *grata persona* to the then authorities. The speech was delivered by M. Jamin, who was one of his successors in the seat he occupied in the section of Physics. M. Bertrand fills his place as Perpetual Secretary. The number of prizes delivered is too great to be reported at full length. We must content ourselves by mentioning the laureates who have worked at questions of general interest. A part of the prize of 6000 francs for progress in efficiency of naval forces has been awarded to the Hydrographic Mission to Tunisia, and to a work of M. Baills on artillery. The Monthyon prize has been awarded to M. Riggenbach, a Swiss engineer, for his railways in mountainous districts. The Poncelet prize, for progress in mathematics, to M. Houel, for the whole of his works. The Lalande prize, for astronomy, to M. Radau, for a memoir on refractions; and the Salz prize, for the same science, to M. Gurzel, for a disquisition on ancient eclipses in order to determine the value of the secular acceleration of the motion of the moon. The Tremont prize has been awarded to M. de Taste for his works on meteorology. M. Marsault has received a gratification of 1500 francs for his studies on lamps for miners. This gentleman is director of the Bessages collieries. The cholera prize was not awarded. M. Durand-Claye, an engineer of the Municipal Service of Paris, who is a strong supporter of the system called "*tout à l'égout*," took a prize of statistics for his researches on diffusion of typhoid fever.

MR. ALEXANDER AGASSIZ has resigned his position as a Fellow of Harvard College, and *Science* states that his resignation was naturally accepted by the Corporation with great reluctance. The *Bulletin* of the University just published contains the formal notes taken at the meeting of October 24, which state "that the wide range of his sympathies and interests, the confidence and affection which he inspired, and the varied information which he possessed both as a man of business and as a man of science, made his services as a fellow of singular value to the University; that his great gifts within the past thirteen years to the scientific departments, and especially to the Museum of Comparative Zoology, which amount to more than half a million of dollars, make him one of the chief benefactors of the University, and entitle him to its profound gratitude."

THE death is announced, at Cannes, of Mr. John Francis Campbell of Islay, at the age of sixty-four years. Mr. Campbell did work in various departments of science. Many years ago he collected the folk-lore of the Western Highlands, and published a large selection of his collections. Mr. Campbell was also a geologist, and in his "Circular Notes" and "Frost and Fire" will be found many geological notes as well as speculations. Quite recently, also, he published a curious book on "Thermography," and he was the inventor, our readers will remember, of the sunshine-recorder at Kew.

WE regret to learn of the death of Mr. Thomas C. Archer, Curator of the Museum of Science and Art, Edinburgh.

M. POYDESSAU, the French engineer who assisted Lieut. Bonaparte Wyse from 1876 to 1878 in his surveys of the Isthmus of Panama, in view of a canal to connect the Atlantic and Pacific, died at Panama on January 7.

Die Natur announces the death of Dr. Friedrich Ritter von Stein, Professor of Zoology in the University of Prague, who is known by a work on Infusoria; and of General Sonklar, one of the first and oldest of Austrian Alpine climbers, whose orographic work in connection with the Austrian Alps has gained him much credit in his native country. He was Professor of Geography at the Military School of Vienna; his latest work was a chart of the rainfall of the Austro-Hungarian Monarchy.

WE learn also of the death of M. Louis Godard, the aëronaut. In 1863 he and his brother Jules went up with Nadar, who still lives, in the monster balloon called *Le Géant*. A breakage in the mechanism necessitated a speedy descent, during which a gust of wind turned the car upside down. The thirteen passengers had barely time to cling to the ropes, and, the grappling irons breaking, the car dragged half a mile on the ground before a landing could be effected. During the siege of Paris Godard left by balloon, and at Tours served on an aëronautic commission. He took no part in recent experiments and discussions on navigable balloons.

ON the afternoon of January 19, we learn from *Science*, the first balloon ascent ever made in the United States solely in the interest of meteorology took place at Philadelphia. Gen. Hazen, chief Signal Officer, U.S.A., recognising the importance and value of a more complete knowledge of the upper atmosphere, entered into a contract some time ago with the well-known aëronaut, Mr. S. A. King, for a number of "trips to the clouds," an ascent to be made at any time on eight hours' notice. The U.S. Signal Service has had this subject under consideration for several years. Prof. Abbe began in 1871 to collect meteorological records made in balloons. In 1872 the records of fifty ascents had been tabulated, studied, and valuable results obtained. In 1876 1000 small balloons were sent with the *Polaris* expedition, to be used in determining the height of the clouds; but, owing to an unfortunate accident, they could not be utilised. At various times the chief Signal Officer has sent observers on balloon excursions which were made for purposes other than scientific. The considerable certainty with which the movement of a storm can now be predicted renders it possible and desirable to make systematic use of the balloon in the study of unusual atmospheric conditions, and the series of ascents just begun is planned with that end in view. Among other things it is desired to determine the difference in the temperature gradient in well-defined "high" and well-defined "low" pressures. For this purpose it is necessary to foretell the arrival of a particular atmospheric condition at Philadelphia, from which place the ascents will be made. This can readily be done so as to give the aëronaut eight hours' notice for the preparation of his balloon, and the observers who accompany him sufficient time to reach Philadelphia from Washington. The first ascent was expected to be rather experimental and suggestive in its character. It was the intention to start at 7 a.m. on the 19th, and a telegram to that effect was sent to Mr. King, who responded that he would be ready. But, owing to the extreme cold, it was found that the balloon could not be handled for filling without danger of cracking; and waiting for the sun to warm it up caused so much delay, that the start was not made till 4.15 p.m. The balloon was the *Eagle Eyrie*, holding 25,000 cubic feet when filled, and having a lifting power of about 1000 pounds. The occupants of the car were Mr. King and Private Hammond, a skilful observer detailed from the office of the Chief Signal Officer for the purpose. Mr. Hammond carried with him a complete outfit for making barometric, thermometric, and hygrometric observations. Owing to the late hour of starting, the observations made were not so numerous as could be desired, although seven complete sets were obtained before darkness rendered further reading impossible. A safe and quiet landing was effected at about 7.30 P.M. near the village of Manahawken, on the New Jersey coast. The greatest height reached was somewhat over one mile. This trial-trip has suggested some modifications in the plans, which will render future ascents more successful. The danger incident to a balloon ascent is greatly over-estimated by many. In the company of an experienced and skilful aëronaut the risk to life and limb is hardly greater than on a railway train or a steamboat. Volunteers for this service are by no means wanting among those connected with the signal service; and Prof. Abbe is so desirous of knowing what is going on "inside of a storm," that he means to make an ascent himself in order to find out.

THE Faculty of Harvard College, by a majority of thirty to two, have decided that Freshmen may be admitted without matriculating in Greek. It is expected that the Classics will soon suffer a further comparative decline, the literature and history of the United States being given greater prominence in the curriculum.

AT a meeting of Convocation of the London University, held on Tuesday, Lord Justice Fry moved, "That, in the opinion of Convocation, the objects of the Association for promoting a Teaching University for London would, if carried into effect by this University, add to its usefulness and importance." His Lordship said that, while he did not wish to cast the slightest slur on the past history of the University, he maintained that there should be a combination of teaching with examination. In his opinion the success of the scheme was inevitable, and it would be far better that it should be carried out by the University than by another examining body. The motion having been carried, the Special Committee was authorised to give effect to the resolutions passed.

THE December number of Prof. Caporali's *Nuova Scienza*, which completes the first year of this remarkable publication, continues to advocate his peculiar system of the universe with unabated vigour and learning. His theory of psychogenesis is here advanced a further stage, and it is now contended not only that psychis is co-eternal with matter, but that it is the true starting-point of all evolution. In the present issue the chief articles are: "Modern Italian Thought," "The Pythagoric Formula of Cosmic Evolution," and "The Anglo-Saxon Anticlerical Evolution." Notwithstanding some curious misconceptions, the last mentioned paper will be read with interest by English students of contemporary thought.

AN extensive Fish Culture Establishment is in course of construction at Delaford Park, Iver, Buckinghamshire, in connection with the National Fish Culture Association. The site is situated close to the River Colne, which is famous for its trout, and affords an abundant supply of fresh water for the purposes required. A number of ponds are being formed upon the most approved scientific principles, in which the various species of *Salmonidæ*, coarse fishes, &c., will be propagated for the benefit

of the community at large. The cultivation of the German carp will also receive considerable attention, this fish being far superior to the English species both as regards its edible qualities and capacity for rapid growth.

WITH a view to effectually prosecuting marine fish culture on sound scientific principles, the National Fish Culture Association have under consideration a scheme for carrying out a series of observations on the temperature of the sea at various stages, in order to obtain a more thorough and concise knowledge of fish, their habits, food, &c. Thermometers for this purpose are in course of manufacture, and will be distributed to those selected for observers under certain rules and regulations. The Duke of Edinburgh is greatly interested in the subject, and has promised his co-operation in furthering the movement, which he considers a most important one.

LARGE consignments of eggs of the *S. levenensis* and white fish have lately been received at the South Kensington Aquarium from the Hon. Prof. Baird, Commissioner of Fish and Fisheries in the United States. All the eggs are in a healthy condition and on the point of incubation. There have been about a dozen premature births amongst them, but, of course, the young fry so born will not live. Prof. Baird has intimated his intention of forwarding a further instalment to South Kensington shortly.

DR. A. WOEIKOFF writes with reference to a note in NATURE for January 29 (p. 298), in which it is stated that the Russian Government are preparing an expedition to Western Siberia to examine the sulphur deposits mentioned by MM. Kalitin and Koushin. These deposits, he states, are not in Western Siberia, but on the so-called old beds east of the Caspian, in a region which it is usual to call Central Asia. It is not exact also to mention the deposits of Tchirkat (*not* Tchirkoto) in Daghestan as the *only* ones till now known in Russia. Sulphur deposits are known in some places near the Volga, and are due to the decomposition of the gypsum so often met with in the Permian formation. Two of these have been worked, one in the eighteenth century, that of Sernaja Gora, on the right bank of the Volga, somewhat above Samara, and another quite recently, that of Sukeewa, about 20 versts above the town of Tetjuchi, government of Kasan.

BEFORE a recent meeting at Annisquam, on the coast of Massachusetts, Mr. J. S. Kingsley described the foundation and work of the Annisquam Marine Laboratory. Prof. Hyatt, of Boston, had been in the habit of inviting some of his students to accompany him to this place during the summer to study the marine forms so abundant there. From the number of applications it appeared that there was a demand for a marine laboratory on the coast near Boston which should be practically free to all. The Woman's Educational Society of Boston became interested in the project, and advanced the money necessary to fit it up. It is under the charge of the Boston Society of Natural History, and was first opened for students in June, 1881. The object of the laboratory, which appears to be open only during the summer vacations at the colleges, is to furnish students with an opportunity of studying marine animals and plants in the best possible manner. Some of those who enter are competent to conduct original investigations, and they are left to follow out any line they may choose. The majority, however, attend to get a foundation and to fit themselves for teaching. Mr. Kingsley describes the aim of the laboratory to be to teach the structure and development of animals, and the methods of study best adapted to produce teachers and investigators. Each student, unless previously qualified, dissects a series of types of the larger forms, such as sea anemones, starfish, clams, lobsters, squid, &c. After this comes a drill in the methods of investigating the embryology of marine forms. The numbers of students range between nine and twenty-one. The laboratory is under the immediate charge of Mr. B. H. Van Vleck. A windmill has lately been added to pump salt water into the building, thus supplying a tank on each of the tables, besides three large aquaria in the centre of the room. The object was to keep the specimens studied alive in confinement—a task of no small difficulty.

AT a meeting held at Edinburgh on Monday it was resolved to hold an international exhibition in that city in the summer of 1886 of industry, science, and art. A committee was appointed to carry out the details.

THE Prince of Wales, as President of the International Inventions Exhibition, has delegated to a Commission selected from among the members of the Executive Council the duty of making arrangements for the effective carrying out of the work of the International Juries. This Commission consists of Sir Frederick Abel (Chairman), Sir P. Cunliffe-Owen, Sir George Grove, Sir E. J. Reed, M.P., Mr. John Robinson, Mr. R. E. Webster, Q.C., with Mr. Trueman Wood (Secretary of the Society of Arts), Secretary of the Commission. His Royal Highness has expressed his wish that, as was the case in the International Health Exhibition last year, the exhibitors should themselves aid in the selection of jurors by submitting the names of those gentlemen whom they may consider most eligible. Exhibitors will, therefore, be asked to send in on a form, to be provided for the purpose, names of gentlemen who might be invited to serve as jurors. The actual selection of jurors will rest with the Jury Commission, who will endeavour to give full weight to the opinions expressed by exhibitors, but will not be restricted to the list of names suggested.

THE Tenth Report of the Boulder Committee of the Royal Society of Edinburgh has come to hand. It is the final report of the Committee appointed in 1871 to collect information regarding erratic blocks or boulders in Scotland, and the Committee do not expect that, by continuing inquiries on the lines available to them, much additional information of importance would be obtained. At all events they regard it as desirable now to arrange their information obtained during the past fourteen years in such a way as to make it more readily accessible. Accordingly they append an abstract of the information in the previous nine reports, so that the present volume may be regarded as a complete record of the work of the Committee. There is also added a "summary of facts, and of inferences apparently deducible from these facts, bearing on the question by what agency boulders were transported to their present sites." The suggested agency is that "of an oceanic current from some north-westerly quarter, bringing masses of floating ice, with boulders upon them, which boulders were deposited on our hills (then submarine) when the ice stranded on these hills." With regard to the question from what country these boulders could have come, and what could have produced the current, the Committee think that though answers might be suggested, they would be going beyond the objects of their appointment in doing so. Their proper province, they say, has been "simply to collect facts bearing on boulders in Scotland, embracing their distribution, their positions, and the agencies probably concerned in their transport. To explain the source or origin of their agencies, or, in other words, to unravel the conditions of the earth's previous history, so as to account for these agencies, is a problem the solution of which must be left to others."

A FULL Report on the East Anglian earthquake of April 22 last, which was probably the most destructive event of its kind in England within the historic period, will be read at the monthly meeting of the Essex Field Club on Saturday next. The Report has been very carefully prepared for the Council by Mr. R. Meldola, with the assistance of many members of the Club and

others. A collection of photographs showing the structural damage will be exhibited. The attendance of those interested in the subjected is invited.

THE last earthquakes in Southern Spain (February 15) were incident with slight subterranean motions in Algiers and in Savoy. The valley of Isère and Chambery principally felt them.

AN exceptionally severe shock of earthquake was felt at Geraldton in Western Australia on January 5. It was preceded by a subterranean rumbling lasting ten seconds. Houses were violently shaken, and the walls rocked, causing much consternation. The sea subsided three feet in a quarter of an hour, returning gradually to its ordinary level. The weather at the time was clear and the temperature cold.

MESSRS. SONNENSCHEIN AND CO. have published a third edition of Dr. Coppinger's "Cruise of the *Alert*."

WE have received from the Royal Museum of Anthropology of Leyden No. 1 of its "Anthropological Notices," by Drs. Serrurier and Jenkate. It deals with the Kroomen of Liberia, arranges the observations in them after the Broca-Topinard method. Only two individuals of the tribe, who had arrived as sailors on board a vessel at Rotterdam, were examined. They came from the region situated between Monrovia and the River Sesters. A plate containing an outline of the feet of each, and of the hand of one, is also added.

THE writer of the letter on "Human Hibernation" in NATURE of February 5 (p. 316) was Col. C. K. Bushe.

THE additions to the Zoological Society's Gardens during the past week include a Serval (*Felis serval* ♂), a Civet Cat (*Viverra civetta* ♀) from West Africa, presented by Mr. T. J. Alldridge, F.Z.S.; a Common Badger (*Meles taxus* ♀), British, presented by Mr. Cuthbert Johnson; two Common Foxes (*Canis vulpes* ♂ ♂), British, presented by Lady Brassey, F.Z.S.; two Pileated Jays (*Cyanocorax pileatus*) from Buenos Ayres, presented by Mr. Theo. Walsh; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, deposited; two Malayan Squirrels (*Sciurus nigrovittatus*) from Malacca, a Four-horned Antelope (*Tetraceros quadricornis* ♀) from India, a Golden-winged Woodpecker (*Colaptes auratus*) from North America, a Pine Grosbeak (*Pinicola enucleator*), European, a Brazilian Teal (*Querquedula brasiliensis* ♀) from Brazil, purchased; four Long-fronted Gerbilles (*Gerbillus longifrons*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE DOUBLE-STAR PIAZZI XIV. 212.—Piazzi first remarked from his own observations between 1800 and 1809, the large proper motion of this star, which was determined by Argelander in vol. vii. of the Bonn Observations to be 2''·015 annually, in the direction 151°·2. "Der Begleiter 8·4m.," he adds, "theilt die Bewegung des Hauptstern; beide bilden also ein System, dass eine ziemlich rasche Aenderung der Distanz und des Positionswinkels zeigt. . . ." The following measures suffice to show the nature of the change in the relative position of the components:—

Herschel and South	1823·3	270°·2	10''·82
Burnham	1881·4	291°·3	15''·38

The most reliable measures may be closely represented by the formulæ—

$$D . \sin P = -12''{\cdot}502 - [8{\cdot}78020] . (t - 1850{\cdot}0)$$
$$D . \cos P = +\ 2''{\cdot}613 + [8{\cdot}96275] . (t - 1850{\cdot}0)$$

But there is one point of interest connected with this star to which attention seems hardly to have been directed—viz. the strange discordances in the estimates of the magnitudes of the components. To illustrate this we may quote the following from a much larger number of estimates recorded:—

At 6h. Greenwich Mean Time

		Star A	Star B
Herschel	1835·45	5½	7
,,	1837·46	6	9
Jacob	1856·24	6	7½
Argelander	1862·89	4·9	8·4
O. Stone	1877·37	7·0	8·5
Flammarion	1877·51	5·5	6·5
O. Stone	1879·47	5·0	8·0
Burnham	1880·32	6·0	8·0
O. Stone	1880·35	8·0	9·5
Burnham	1881·36	6·5	8·0

Gould has 6·3 and 7¼. The star is not in Argelander's *Uranometria*, nor has Heis got it. Argelander made a difference of 3½ magnitudes in 1862-63, Flammarion in 1877 rated the fainter star only one magnitude below the other. The difference between Burnham and O. Stone at nearly the same time in 1880 may have been due to atmospheric conditions at Cincinnati, but the star appears to be worth watching for variability; compare Argelander in 1862 with Burnham in 1881 or with Gould.

WOLF'S COMET.—The following ephemeris for 6h. G.M.T. is founded upon one for Berlin midnight, calculated from Prof. Krueger's last orbit, by Dr. Lamp, of Kiel:—

	R.A. h. m. s.	Decl. ° '	Log. distance from Earth	Log. distance from Sun
March 2	3 7 13	−0 9·7	0·3243	0·2752
3	9 31	−0 2·6		
4	11 49	+0 4·4	0·3296	0·2776
5	14 7	0 11·3		
6	16 24	0 18·2	0·3348	0·2800
7	18 42	0 25·0		
8	20 59	0 31·8	0·3400	0·2825
9	23 16	0 38·5		
10	25 33	0 45·3	0·3451	0·2849
11	27 50	0 52·0		
12	30 6	0 58·6	0·3502	0·2873
13	32 22	1 5·1		
14	3 34 38	+1 11·6	0·3553	0·2897

Mr. J. I. Plummer observed the comet for position on February 18, notwithstanding the presence of a 3½ days' moon.

ASTRONOMICAL PHENOMENA FOR THE WEEK, 1885, *MARCH* 1-7

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 1

Sun rises, 6h. 47m.; souths, 12h. 12m. 27·8s.; sets, 17h. 39m.; decl. on meridian, 7° 24' S.: Sidereal Time at Sunset, 4h. 18m.

Moon (Full at 4h.) rises, 17h. 12m.*; souths, 0h. 1m.; sets, 6h. 38m.; decl. on meridian, 6° 15' N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on Meridian ° '
Mercury	6 42	11 36	16 31	13 16 S.
Venus	6 23	11 12	16 2	14 19 S.
Mars	6 45	11 59	17 13	9 50 S.
Jupiter	16 19	23 29	6 39*	12 50 N.
Saturn	10 23	18 27	2 31*	21 38 N.

* Indicates that the rising is that of the preceding, and the setting that of the following nominal day.

Occultation of Star by the Moon

March	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
7	θ Libræ	4½	0 52	2 2	30 240

Phenomena of Jupiter's Satellites

March	h. m.		March	h. m.	
1	0 10	II. ecl. reap.	6	1 20	I. ecl. reap.
2	17 53	II. tr. egr.		4 5	II. tr. ing.
3	2 6	III. occ. disap.		19 19	III. tr. egr.
4	4 16	I. occ. disap.		20 0	I. tr. ing.
5	1 34	I. tr. ing.		22 20	I. tr. egr.
	3 54	I. tr. egr.	7	19 49	I. ecl. reap.
	22 42	I. occ. disap.		23 4	II. occ. disap.

☞ The occultations of stars and phenomena of Jupiter's satellites are such as are visible at Greenwich.

Saturn, March 1.—Outer major axis of outer ring = 42″·3; outer minor axis of outer ring = 19″·2; southern surface visible.

March	h.	
6 ...	10 ...	Venus at greatest distance from the Sun.
7 ...	14 ...	Mercury in conjunction with and 1° 3′ south of Mars.

GEOGRAPHICAL NOTES

IT is stated that the King of the Belgians is conferring with M. Martinie, president of the French Geographical Society, on the subject of the formation of an International Geographical Society.

THE last issue of the *Izvestia* of the Eastern Siberian branch of the Russian Geographical Society contains an interesting paper by M. Doubrof on his journey to Mongolia. The author, accompanied by only one man, has explored the upper course of the Selenga and reached the hitherto unvisited source of this great tributary of Lake Baikal. Unhappily, on his return journey he was prevented from following the exploration of its middle course, the whole journey having been undertaken at so small an expense that the author had sharply to calculate every rouble he was able to expend. The want of barometrical observations on the high tablelands of the Upper Selenga is especially regretable, and it is not wholly compensated by a mere topographical description. A table of the times of the freezing of many Siberian rivers and of the breaking of the ice is given in the same fascicule, as also several notes on the Lena meteorological station—already old—and on the Yakutsk province.

THE trade in children within the province of Yakutsk is the subject of an interesting note in the same journal. The Irkutsk Geographical Society had received a note from one of its members, who thus depicted the lot of girls within the province: In the last century the poorest Yakute who had no means of supporting a large family, took his new-born child in a covering of birch-bark and hung it on a tree in the forest to die from hunger. But the richer Russian merchants began to buy children from their poorer Yakute clients, and so several Russians purchased whole families of servants. This custom induced the Yakute communities to take care of the poorest children, and the community was bound to feed them, under the name of *Kumolan* children, who spent three days in the houses of the richer members of the community, two days in those of the moderately wealthy, and one day with the poorest. But of late the custom has arisen of selling children, and especially girls, to Olekminsk merchants, who sell them further to the Yakutes and Tunguses of the Olekminsk district. The parents sell girls for thirty to forty roubles (3*l.* to 4*l.*), and in Olekmir they are re-sold for sixty roubles, sometimes eighty roubles. Of course this trade is made under the cover of "taking children to bring up." The Irkutsk Society having taken interest in this communication, it has received information from Yakutsk authorities, and from a well-known student of Yakute life, M. Gorokhoff. It appears from these communications that such trade really exists, the chief impulse to it being given, less by the work a purchased girl might do than by the possibility of receiving for her the *kalym*, that is, the money paid by men for purchasing a wife. Woman labour is at so low a price that one might have a woman in his household and pay her half a piece of cotton, "for a shirt," per year. But the *kalym* reaches very high prices. One rich Yakute has recently sold his daughter to a Tungus for 3000 reindeer, and the same price was recently given by a half-idiotic Yakute for the daughter of another Yakute. Middendorff quotes also several instances of a very high *kalym* paid for girls, its average being about 500 roubles. When a Russian priest sold a girl whom he had educated for five sables and ten skins, it was considered as a very low price. Altogether, the *kalym* is the chief cause of maintaining the trade in girls, together with the gradual impoverishment of the Yakutes.

THE *Japan Gazette* publishes a brief statement from Mr. Gowland, technical adviser to the Imperial Mint at Osaka, on his observations during a recent journey through part of Corea. He spent ten days at Seoul, the capital, and twenty days on the overland route between that place and the port of Fusan. He did not observe any indication of mineral wealth. There were no signs of mines, and nothing beyond doubtful indications of mineral veins in one or two places. There are no mountains exceeding about 4000 feet in highest elevation, and no characteristic volcanic cones. The central range was crossed by a pass 2300 feet above the sea-level. The forests were of no great extent, but very extensive tracts of cultivated ground, evidently yielding a large surplus production of rice, barley, and beans, were noticeable throughout. There was a marked absence of any manufacturing industry, or of indications that anything beyond food-products receives attention. The traffic on the roads was limited to that between neighbouring districts only, and this was very little. The beasts of burthen employed were rarely horses, frequently bullocks, and chiefly men. There is a total absence of any signs of wealth, and the resources of the country appear to lie solely in agriculture. There is no money, and no prospects of any foreign trade.

THE last number of *Le Mouvement Géographique* has some interesting information about the celebrated first letter from Columbus. All interested in the early history of America know of the different editions of this letter, which was first published in 1493. Bibliographers mention seven of them: (1) one in Rome by Stephen Plannck; (2) one called the Libri Lennox; (3) one in Rome by Eucharius Argenteus; (4) a second by Plannck at Rome; (5) a Paris copy; (6) a second Paris copy; (7) one discovered in Turin by Harisse. To these an eighth has just been added by Ruelens, who discovered the only copy of it known to exist in the Royal Library at Brussels. It is a small pamphlet of four leaves in quarto, of thirty-eight lines, without figures or signature, in semi-Gothic characters. It appears to have been purchased between 1815 and 1830 by the Royal Library. Its title is: "Epistola Christophori Colom: cui etas nostra multum debet." The title then goes on as follows: "De Insulis Indie supra Gangem puper [for ruper] inventis. Ad quas perquirendas octavo ante mense auspiciis et ere inuictissimi Ferdinandi Hispaniorum Regis missus fuerat: ad magnificum Dominum Raphaelem Sauxis: ejusdem serenissimi Regis Tesaurarium missa: quam nobilis ac litteratus vir Aliander de Cosco ab Hispano idiomate in latinum convertit: tertio Maii MCCCC. XC. III. Pontificatus Alexandri Sixti Anno primo." Although the little pamphlet does not bear the name of a publisher, M. Ruelens, by comparing the works of the great Flemish printers, has discovered that Martens was the person. This individual distinguished himself among all his fellows about the end of the fifteenth century, at Antwerp, by his intelligent and progressive character. He was a great publisher of his day; he issued more than fifty writings of Erasmus, More's "Utopia," works of Savonarola, and many others. Facsimiles of the letter have been printed by M. Ruelens, and fifty of them, numbered and paged, are offered for sale. The discovery of this relic of geographical discovery, as well as of early Flemish printing, is an event of great interest.

THE *Echo du Japan* reports the arrival in Japan, at the beginning of the year, of M. Joseph Martin, a French traveller, who has just been exploring the parts of Siberia hitherto very little known. His principal journey was from the Lena to the Amoor, across the Stanowai chain of mountains. During his explorations he was able to make geographical and geological collections, which are intended for the Paris museums. In consequence of hardships endured on the journey, two of his native followers died and one lost his reason.

IN a paper read before the Statistical Society on the 17th inst. Sir Richard Temple endeavoured to check the various official returns of the population of China by applying the results obtained from the population statistics of British India. The various statements made by the Chinese Government as to the numbers of people under its rule show violent fluctuations, those of the last century and a half varying between 436 and 363 millions. These returns, as Prof. Douglas pointed out, varied with the purposes for which the enumerations were made. China proper and India, said Sir Richard Temple, are about the same area—a million and a half of square miles. Both countries are under similar conditions, physical, technical, climatic, geographical. In both there is a strong tendency to multiplication of the race. In both the population loved to congregate in favoured districts, to settle down and multiply there till the land could scarcely sustain the growing multitudes, and to leave the less favoured districts with a scanty though hardy population. The average population of the whole of India is 184 to the square mile, and if this average be applied to China (exclusive of the Central plateau) it gives a population of 282,191,600 souls. The writer then compared, one by one, the eighteen

provinces of China proper with the districts in India corresponding nearly in physical characteristics and cultivable area, and, summarising these computations, he found that, over a total area of 1,500,650 square miles, the population, according to this estimate from the Indian averages, would be 282,161,923, or, say, 183 persons to the square mile, while the latest official returns obtained from China show 349,885,386, or 227 inhabitants to the square mile. The general conclusion, he said, might be that the latest Chinese returns, though probably in excess of the reality, did not seem to be extravagant or incredible on the whole if tested by the known averages of the Indian census.

THE FORMS OF LEAVES[1]

SIR JOHN LUBBOCK said that, greatly as we all appreciated the exquisite loveliness of flowers, we must admit that the beauty of our woods and fields was as much due to the marvellous grace and infinite variety of foliage. How is this inexhaustible richness of forms to be accounted for? Does it result from an innate tendency of the leaves in each species to assume some particular shape? Has it been intentionally designed to delight the eyes of man? Or has it reference to the structure and organisation, the wants and requirements of the plant itself?

Now, if we consider first the size of the leaf, we shall find that it is regulated mainly with reference to the thickness of the stem. This was shown, for instance, by a table giving the leaf area and the diameter of stem of the hornbeam, beech, elm, lime, Spanish chestnut, ash, walnut, and horse-chestnut. When strict proportion is departed from, the difference can generally be accounted for.

The size once determined exercises much influence on the form. For instance, in the beech the leaf has an area of about 3 square inches. The distance between the buds is about 1¼ inches, and the leaves lie in the general plane of the branch, which bends slightly at each internode. The basal half of the leaf fits the swell of the twig, while the upper half follows the edge of the leaf above, and the form of the inner edge being thus determined decides that of the outer one also. In the lime the internodes are longer and the leaf consequently broader. In the Spanish chestnut the stem is nearly three times as stout as that of the beech, and consequently can carry a larger leaf surface. But the distances between the buds are often little greater than those in the beach. This determines, then, the width, and, by compelling the leaf to lengthen itself, leads to the peculiar form which it assumes. Moreover, not only do the leaves on a single twig admirably fit one another, but they are also adapted to the ramification of the twigs themselves, and thus avail themselves of the light and air, as we can see by the shade they cast without large interspaces or much overlapping.

In the sycamores, maples, and horse-chestnuts the arrangement is altogether different. The shoots are stiff and upright, with leaves placed at right angles to the plane of the branch, instead of being parallel to it. The leaves are in pairs, and decussate with one another, while the lower ones have long petioles, which bring them almost to the level of the upper pairs, the whole thus forming a beautiful dome.

For leaves arranged as in the beech, the gentle swell at the base is admirably suited; but in a crown of leaves, such as those of the sycamore, space would be thereby wasted, and it is better that they should expand at once, as soon as their stalks have carried them free from the upper and inner leaves. Hence we see how beautifully the whole form of these leaves is adapted to the mode of growth and arrangement, of the buds in the plants themselves.

In the black poplar the arrangement of the leaves is again quite different. The leaf-stalk is flattened, so that the leaves hang vertically. In connection with this it will be observed that, while in most leaves the upper and under surfaces are quite unlike, in the black poplar, on the contrary, they are very similar. The stomata or breathing-holes, moreover, which in the leaves of most trees are confined to the under surface, are in in this species nearly equally numerous on both. The "compass" plant of the American prairies, a yellow composite not unlike a small sunflower, is another plant with upright leaves, which, growing in the wide open prairies, tend to point north and south, thus exposing both surfaces equally to the light and heat. It was shown by diagrams that this position also affected the internal structure of the leaf.

In the yew the leaves are inserted close to one another, and are long and linear; while in the box they are further apart and broader. In the Scotch fir the leaves are linear, and 1½ inch long; while in other pines, as, for instance, the Weymouth, the stem is thicker and the leaves longer.

In the plants hitherto mentioned one main consideration appears to be the securing of as much light as possible; but in tropical countries the sun is often too powerful, and the leaves, far from courting, avoid the light. The typical acacias have primate leaves, but in most Australian species the true leaves are replaced by a vertically flattened leaf-stalk. It will be found, however, that the seedlings have leaves of the form typical in the genus. Gradually, however, the leaf becomes smaller and smaller, until nothing is left but the flattened leaf-stalk or phyllode. In one species the plant throughout life produces both leaves and phyllodes, which give it a very curious and interesting appearance. In eucalyptus, again, the young plant has horizontal leaves, which in older ones are replaced by scimitar-shaped phyllodes. Hence the different appearances of the young and old trees which must have struck every visitor to Algiers or the Riviera.

We have hitherto been considering mainly deciduous trees. In evergreens the conditions are in many respects different. It is generally said that leaves drop off in the autumn because they die. This, however, is not strictly correct. The fall of the leaf is a vital process connected with a change in the cellular tissues at the base of the leaf-stalk. If the leaves are killed too soon they do not drop off. Sir John illustrated this by some twigs which he had purposely broken in the summer; below the fracture the leaves had been thrown off, above they still adhered, and so tightly that they could support a considerable weight. In evergreen trees the conditions are in many respects very different. It is generally supposed that the leaves last one complete year. Many of them, however, attain a much greater age: for instance, in the Scotch fir, three or four years; in the spruce and silver, six or seven; in the yew even longer. It follows from this that they require a tougher and more healthy texture. When we have an early fall of snow our deciduous trees are often much broken down; glossy leaves have a tendency to throw it off, and thus escape, hence evergreen leaves are very generally smooth and glossy. Again, evergreen leaves often have special protection either in an astringent or aromatic taste, which renders them more or less inedible; or by thorns and spines. Of this the holly is a familiar illustration; and it was pointed out that in old plants above the range of browsing quadrupeds, the leaves tend to lose their spines and become unarmed. The hairs on leaves are another form of protection; on herbs the presence of hairs is often associated with that of honey, as they protect the plants from the visits of creeping insects. Hence perhaps the tendency of water species to become glabrous, *Polygonum amphibium* being a very interesting case, since it is hairy when growing on land, and smooth when in water. Sir John then dealt with cases in which one species mimics another, and exhibited a striking photograph of a group of stinging nettles and dead nettles, which were so much alike as to be hardly distinguishable. No one can doubt that the stinging nettle is protected by its poisonous hairs, and it is equally clear that the innocuous dead nettle must profit by its similarity to its dangerous neighbour. Other similar cases were cited.

He had already suggested one consideration, which in certain cases determined the width of leaves; but there were others in which it was due to different causes, one being the attitude of the leaf itself. In many genera with broad and narrow-leaved species, drosera and plantago, for instance, the broad leaves formed a horizontal rosette, while the narrow ones were raised upwards. Fleshy leaves were principally found in hot and dry countries, where this peculiarity had the advantage of offering a smaller surface, and therefore exposing the plant less to the loss of water by evaporation.

Sir John then passed to aquatic plants, many of which have two kinds of leaves: one more or less rounded, which floats on the surface; and others cut up into narrow filaments, which remain below. The latter thus presents a greater extent of surface. In air, however, such leaves would be unable to support even their own weight, much less to resist any force such as that of the wind. In perfectly still air, however, for the same reason, finely divided leaves may be an advantage, whereas in

[1] Abstract by the Author of a Lecture delivered at the Royal Institute, Feb. 13 by Sir John Lubbock, Bart., M.P., D.C.L., LL.D., F.R.S., &c.

comparatively exposed situations more compact leaves may be more suitable. It was pointed out that finely cut leaves are common among low herbs, and that some families which among the low and herbaceous species have such leaves, in shrubby or ligneous ones have leaves more or less like those of the laurel or beech.

Much light is thrown on the subject by a study of the leaves of seedlings. Thus the furze has at first trifoliate leaves, which gradually pass into spines. This shows that the furze is descended from ancestors which had trifoliate leaves, as so many of its congeners have now. Similarly, in some species which when mature have palmate leaves, those of the seedling are heart-shaped. He thought that perhaps in all cases the palmate form was derived from the heart-shaped. He then pointed out that if there were some definite form told off for each species then a similar rule ought to hold good for each genus. The species of a genus might well differ more from one another than the varieties of any particular species; the generic type might be, so to say, less closely limited; but still there ought to be some type characteristic of the genus.

He took, then, one genus, that of Senecio (the groundsel). Now in addition to Senecios more or less resembling the common groundsel, there were species with leaves like the daisy, bushy species with leaves like rosemary and the box, small trees with leaves like the laurel and the poplar, climbing species like the convolvulus and bryony. In fact the list is a very long one, and shows that there is no definite type of leaf in the genus, but that the form in the various species depends on the condition of the species. From these and other considerations he concluded that the forms of leaves did not depend on any inherent tendency, but to the structure and organisation, the habits and requirements of the plant. Of course it might be that the present form had reference to former and not to present conditions. Nor did it follow that the adaptation need be perfect. The tendency existed, just as water tends to find its level. This rendered the problem all the more complex and difficult.

The lecture was illustrated by numerous diagrams and specimens, and Sir John concluded by saying the subject presented a wide and interesting field of study, for if he were correct in his contention every one of the almost infinite forms of leaves must have some cause and explanation.

SCIENTIFIC SERIALS

Journal of the Russian Chemical and Physical Society, vol. xvi. fasc. 8.—On the oxidation of acetones, by E. Wagner (first paper dealing with their behaviour towards chromic acid).—On the specific volumes of chlorine, iodine, and bromine in organic compounds, by M. Schalfejeff (second paper). For chlorine they gradually rise with the increase of the number of equivalents entering into combination, gradually reaching 21, 24, and 27; for bromine they are 24, 27, and 30; and for iodine, 26 to 27.—Addition of methylamine to methylglycidic acid, by M. Zelinsky.—On Astrakhanite, by W. Markovnikoff.—On the influence of the lineary compression of iron, steel, and nickel rods on their magnetism, by P. Bakmetieff. From a varied series of experiments the author arrives at a series of conclusions, showing that compression of iron rods exercises a very notable influence on their magnetisation, and that the phenomena depend upon the rods having been, or not, formerly submitted to repeated compression; all kinds of iron and steel display the influence of compression—soft iron and steel at a higher degree than hard iron and steel. The theory of rotating molecular magnets would explain all observed phenomena.—On an amperometre based on the electrothermic phenomenon of Pelletier, by N. Hesehus.—On the regular forms taken by powders, by Th. Petrushevsky (second paper dealing with the shapes taken by heaps of powders on surfaces limited by curves, or polygones with entering angles).—Also on the dilatation of liquids; an answer to Prof. Arenarius, by D. Mendeleeff.—An answer to M. Rogovsky, by B. Stankewitsch.—An answer to M. Sokoloff, by M. Bardsky, being a further mathematical inquiry into the forces of molecular attraction.—An answer to M. Petroff, by M. Kraevitsch.—We notice an innovation in this fasciculum of the *Journal*. It contains detailed minutes of the proceedings of the Physical and Chemical Section of the Moscow Society of Lovers of Natural Science.

Sitzungsberichte der Physicalisch-medicinishen Societät zu Erlangen, No. 16, October, 1883, to October, 1884.—Remarks on the phenomenon of phosphorescence in connection with the description of an instrument designed for studying the effect of the various spectral rays, and especially the ultra-red on phosphorising substances, by E. Lommel.—On the fluorescence of calcspar, by E. Lommel.—On the reduction of algebraic differential expressions to normal forms, by M. Noether.—Contributions to the knowledge of the Chytridiaceæ and other fungoid organisms, with thirty-seven illustrations, by D. C. Fisch.—On the malaria and intermittent fevers of the Erlangen district, by Prof. F. Penzoldt.—On the presence of microscopic organisms in the tissues of animals in the normal state, by Dr. Hauser.—Test of the sensitiveness of the visual organ to direct and oblique luminous rays, by Dr. Louis Wolffberg.—On algebraic differential expressions, and on Jacobi's reverse problem, by M. Noether.—On the systematic position of the yeast fungus, by M. Reess.—On two new species of Chytridiaceæ, by C. Fisch.—On the nerves of temperature and touch in the animal system, by J. Rosenthal.—On a means of determining the quantity of carbonic acid present in the atmosphere of rooms, by J. Rosenthal.—On the phenomenon of Uræmia, by Dr. R. Fleischer.—Toxicologic researches from the physiological standpoint, by J. Rosenthal.—On vertigo caused by intestinal affections, by W. Leube.—Experiments on the hatching of bird's eggs whose shells had suffered lesion, by Prof. L. Gerlach.—On Oidema, by Dr. R. Fleischer.—On the surgical operation of opening the mastoid process, by Dr. W. Kiesselbach.—On the life-history and pathological properties of a species of bacteria causing putrefaction, by Dr. G. Hauser.—On the histiology of primary carcinom in the osseous system, by Dr. von Düring.—On a case of lingual tuberculosis, by Dr. Ernst Graser.—On the after-treatment of external urethrotomy, by H. Knoch.

Rivista Scientifico-Industriale, December 31, 1884.—On the electric conductivity of the alcoholic solutions of some chlorides, by Dr. Joseph Vicentini.—Memoir on the variations in the electric resistance of solid and pure metal wires according to the temperature (continued), by Prof. Angelo Euro.

SOCIETIES AND ACADEMIES

London

Royal Society, February 12.—"On Underground Temperatures, with Observations on the Conductivity of Rocks, on the Thermal Effects of Saturation and Imbibition, and on a Special Source of Heat in Mountain Ranges." By Joseph Prestwich, M.A., F.R.S., Professor of Geology in the University of Oxford.

The author remarks on the difference of opinion between physicists and geologists respecting the probable thickness of the outer crust of the earth—the former on the strength of its great rigidity and the absence of tides, contending for a maximum thickness and comparative solidity of the whole mass; while the latter, in general, on the evidence of volcanic action, the crumpling and folding of the strata in mountain ranges, its general flexibility down to the most recent geological times, and the rate of increase of temperature in descending beneath the surface, contend for a crust of minimum thickness as alone compatible with these phenomena.

The question of underground temperature, which is a subject equally affecting the argument on both sides, had engaged the author's attention in connection with an inquiry respecting volcanic action, and he was induced to tabulate the results to see how far the usually received rates of increase were affected by various interfering causes—not that most of them had not received due attention, but it was a question whether sufficient allowance had been made for them.

Although Gensanne's first experiments were made in 1740, and others were subsequently made by Daubuisson, Saussure, and Cordier, in coal and other mines, it was not until the construction of deep artesian wells commenced in the second quarter of this century, and Walferdin introduced his overflow thermometer, and precautions were taken against pressure, that the more reliable observations were made and admirably discussed by Arago. The Coal Commission of 1866 collected a mass of important evidence bearing on the question, and in 1867 a Committee of the British Association was appointed to collect further information. Under the able superintendence of Prof. Everett, a series of valuable experiments with improved instruments has

been made, and full particulars published in the *Annual Reports* of 1868-83.

But notwithstanding the precautions taken, and the accuracy of the experiments, they present very wide differences in the thermometric gradient, ranging from under 30 to above 120 feet per degree F. Consequently different writers have adopted different mean values. On the Continent one of 30 m. per degree C. has been commonly adopted, while in this country some writers have taken a mean of 50 feet per degree, and others of 60 feet or more. The object which the author has in view is to see whether it is not possible to eliminate the more doubtful instances, and to bring the probable true normal gradient within narrower limits. In so doing he confines himself solely to the geological side of the inquiry.

In a general list, Table I., he gives all the recorded observations in the order of date. The list embraces observations at 530 stations in 248 localities. The most reliable of these he classifies under three heads in Tables II., III., and IV.

(1) Coal mines.
(2) Mines other than coal.
(3) Artesian wells and bore-holes.

To which tunnels are added in a supplement.

The author then proceeds to point out that the gradients given in many of the earlier observations were wrong in consequence of neglecting the height of the surface, and from the exact mean annual temperature of the locality not being known. They also differed amongst themselves from taking different surface temperatures, and starting from different datum levels. To these he endeavours to assign a uniform value.

The essential differences in the results in several tables depend, however, upon dissimilar geological conditions, which unequally affect the conductivity of the strata, and disturbing causes of different orders. In the mines the latter are :—

(1) The currents established by ventilation and convection.
(2) The circulation of underground waters.
(3) Chemical reactions.
(4) The working operations.

And in artesian wells—

(1) The pressure of the water on the thermometers.
(2) Convection currents in the column of water.

In the latter experiments pressure has been thoroughly guarded against, but against the subtle influence of the other causes, though long known, it is more difficult to guard.

Coal Mines.—The author then proceeds *seriatim* with each subject, commencing with coal-mines. In these he shows that ventilation and convection currents have rendered many of the results unreliable, as he shows to have been the case in the well-known instance of the Dukinfield coal pit. The circulation of air in coal pits varies from 5000 to 150,000 cubic feet per minute, and tables are given to show how this variously affects the temperature of the coal at different distances from the shaft, *though on the same level*. As a rule, the deeper the pit the more active is the ventilation, and therefore the more rapid the cooling of the underground strata. In some pits the indraughted air has been known to form ice, not only in the shaft, but icicles in the mine near the shaft.

The cooling effects of ventilation are shown to begin immediately that the faces of the rock and coal are exposed, and as the hotter (and deeper) the pit, and the more gassy the coal, the more active is the ventilation, so these surfaces rapidly undergo a cooling until an equilibrium is established between the normal underground temperature and the temperature of the air in the gallery. Judging by the effects of the diurnal variations on the surface of the ground, it is clear that an exposure of a few days must, when there is a difference of 10° to 12° or more between the air in the gallery and the normal temperature of the rock, tell on the exposed coal and rock to the depth of 3 to 4 feet—the usual depth of the holes in which the thermometers are placed. The designation of "fresh open faces" is no security, as that may mean a day or a week, or more. The author considers also that so far from the length and permanence of the experiment affording security, he is satisfied on the contrary that those experiments in which it is stated that the thermometer has been left in the rock for a period of a week, a month, or two years without any change of temperature, affords *prima facie* evidence of error, inasmuch as it shows that the rock has so far lost heat as to remain in a state of equilibrium with the air at the lower temperature in constant circulation.

Another cause of the loss of heat which requires some notice is the escape of the gas, which exists in the coal either in a highly compressed, or, as the author thinks more probable, in a liquid state. A strong blower of gas has been observed to render the coal sensibly cooler to the touch. In another case whereas the temperature of the coal at the depth of 1269 feet was 74° F., at the greater depth of 1588 feet in a hole with a blower of gas it was only 62°. One witness observed that "the coal gives out heat quicker than the rock." There is generally a difference of 2° or 3° between them.

On the other hand, the coal and rocks when crushed and in "creeps" acquire a higher temperature owing to the liberation of heat.

The effects of irregularities of the surface on the underground isotherms, although unimportant in many of our coal-fields, produce very decided results in the observations on the same level in the mines among the hills of South Wales. Sections are given to show how the temperature rises under hills and falls under valleys, showing that it is often essential to know not only the depth of the shaft but the depth beneath the surface at each station where the experiments are made.

The author therefore considers that to assign a value to an observation we should know (1) height of pit above sea-level; (2) the exact mean annual temperature of the place; (3) depth beneath the surface of each station; (4) distance of the stations from the shaft; (5) temperature and columns of air in circulation; (6) length of exposure of face; (7) whether or not the coal is gassy. The dip of the strata and the quantity of water are also to be noted.

Very few of the recorded observations come up to this standard, and the author has felt himself obliged to make a very restricted selection of cases on which to establish the probable thermometric gradient for the coal strata. Amongst the best observations are those made at Boldon, North Seaton, South Hetton, Rosebridge, Wakefield, Liége, and Mons. These give a mean gradient of 49½ feet for each degree F. The bore-holes at Blythswood, South Balgray, and Creuzot give a mean of 50·8 feet.

Mines other than Coal.—The causes affecting the thermal conditions of these mines are on the whole very different to those which obtain in coal mines. Ventilation affects both, but in very unequal degrees. In mineral mines it is much less active, and the cooling effects are proportionately less. On the other hand the loss of heat by the underground waters in mineral mines is very important. In some mines in Cornwall, the quantity of water pumped up does not exceed 5 gallons, while in others it amounts to 200 gallons per minute. The Dolcoath mine used to furnish half a million gallons of water in the twenty-four hours, while at the Huel Abraham mine it reached the large quantity of above 2,000,000 gallons daily. The rainfall in Cornwall is about 46 inches annually, and of this about 9 inches pass underground. In the Gwennap district, where 5500 acres were combined for drainage purposes, above 20,000,000 gallons have been discharged in the twenty-four hours from a depth of 1200 feet. This water issues at temperatures of from 60° to 68°, or more than 12° above the mean of the climate, showing how large must be the abstraction of heat from the rocks through which the waters percolate.

Hot springs are not uncommon in these mines. They are due to chemical decomposition, and to water rising in the lodes and fissures from greater depths. The decomposition which goes on in the lodes near the surface, and whereby the sulphides of iron and copper are reduced ultimately to the state of peroxides and carbonates of those metals, is a permanent cause of heat, especially apparent in the shallower mines. On the other hand, where the surface waters pass rapidly through the rocks, they lower the temperature and give too low readings.

While ventilation, therefore, reduces the rock temperature, the water which percolates through the rock, and more especially through the veins and cross-courses, sometimes raise, and at other times lower, the temperature of the underground springs. Mr. Were Fox, who for many years made observations on the underground temperature of the Cornish mines, gave the preference to the rocks; while Mr. Henwood, an observer equally experienced and assiduous, considered that the underground springs gave surer results. Both were of course fully alive to all the precautions that in either case it was necessary to take to guard against these interferences.

Taking ten of the most reliable of Mr. Henwood's observations at depths of from 800 to 2000 feet, the mean gives a thermometric gradient of 42·4 feet per degree, but Mr. Henwood himself gives us the mean of 134 observations to the depth of

1200 feet, a gradient of 41·5 feet to the experiments in granite, and of 39 feet to those in slate.

Taking the experiments of Mr. Fox in eight mines, varying in depth from 1100 to 2100 feet, the mean of the experiments made in the rock gave a gradient of 43·6 feet per degree, or for the mean of the two observers we have a gradient of 43 feet per degree.

For the foreign mines, in the absence of fuller data, and especially failing in information of the depth of the station beneath the surface, which in the hilly district of Freiberg and Hungary introduces an element of great uncertainty, it is impossible to arrive at any safe conclusion.

Artesian Wells and Borings.—This class of observations presents results much more uniform, and whereas the mines observations were made, the one in crystalline, and the other in unaltered Palæozoic rocks, the wells are, with few exceptions, either in the softer and less coherent rocks of Cretaceous, Jurassic, and Triassic age, which are much more permeable, and, as a rule, much less disturbed.

The causes of interference are mainly reduced to pressure on the instruments and convection currents. The early experiments, where no precautions were taken against these, are, with few exceptions, unreliable, and must be rejected. The larger the bore-hole, the greater the risk of convection-currents, and Prof. Everett has shown that in many cases of deep and large artesian borings, the water which lodges in them is reduced to a nearly uniform temperature throughout the whole depth by the action of these currents. In the deep boring at Sperenberg, before the introduction of plugs to stop these currents, it was found that the temperature near the top of the bore was rendered 4°·5 F. too high, and at the bottom, at a depth of 3390 feet, 4°·6, if not 6°·7, too high by the currents.

Taking the bore-holes in which the water does not overflow, and where, owing to the precautions against these sources, such as those of Kentish Town, Richmond, Grenelle, Sperenberg, Pregny, and Ostend, we get a mean gradient of 51·9 feet per degree.

Overflowing artesian wells should, if we were sure of all the conditions, give the best and most certain results. Taking those where the volume of water is large, and the observations made by competent observers, as in the case of the wells of Grenelle, Tours, Rochefort, Mondorff, Minden, and others, we obtain a mean of 50·2 feet, or, taking the two sets of wells, of 51 feet per degree.

The author, however, points out a source of possible error in those wells, arising from a peculiarity of tubage which requires investigation, and owing to which he thinks the water may suffer a loss of heat in ascending to the surface.

With respect to the extra-European wells, more particulars are required. It may be observed, however, that the wells in the Sahara Desert, which were made by an experienced engineer accustomed to such observations, the mean of eleven overflowing wells, at depths of from 200 to 400 feet, gave 36 feet per degree.

Tunnels.—For the Mont Cenis Tunnel, allowing for the convexity of the surface, Prof. Everett estimates the gradient at 79 feet, and for the St. Gothard, 82 feet per degree. But Dr. Stapff found in the granite at the north end of the tunnel a much greater heat and more rapid gradient, for which there seemed no obvious explanation. Though this axis of the Alps is of late Tertiary date, the author points out that it cannot be due to the protrusion of the granite, as the Swiss geologists have shown that the granite was in its present relative position and solidified before the elevation of this last main axis of the Alps, and he suggests that the higher temperature may be a residue of the heat caused by the intense lateral pressure and crushing of the rocks which accompanied that elevation, for in the crushing of a rigid material such as rock almost the entire mechanical work reappears as heat.

Conductivity of the Rocks. Effects of Saturation and Imbibition.—Some of the apparent discrepancies in the thermometric gradients are no doubt due to differences in the conductivity of the rocks. Applying the valuable determinations of Profs. Herschel and Lebour to the groups of strata characterising the several classes of observations, the following results are obtained :—

	Mean conductivity k	Mean resistance r
(1) Carboniferous strata	·00488	275
(2) Crystalline and schistose rocks	·00546	184
(3) Triassic and Cretaceous strata	·00235	465

From this it would appear that the conductivity of the rocks associated with the mineral mines is twice as great as that of the artesian wells class. But all the experiments, with the exception of three or four, were made with blocks of dried rocks, and those showed a very remarkable difference ; thus, for example, dry New Red Sandstone gave k0·00250, whereas when wet it was increased to k0·00600. The author remarks that as all rocks below the level of the sea and that of the river valleys are permanently saturated with water, dry rocks are the exception and wet rocks the rule in nature, consequently the inequalities of conductivity must tend to disappear. The power of conduction is also greater along the planes of cleavage or lamination than across them, and therefore the dip of the strata must also exercise some influence on the conductivity of different rocks and "massifs." With respect to the foliated and schistose rocks, M. Jannettaz has shown that the axes of the thermic curve along and across the planes of foliation and cleavage are in the following proportions :—

Gneiss of St. Gothard	1 : 1·50
Schists of Col Voza	1 : 1·80
Cambrian slates, Belgium	1 : 1·98

This cause will locally affect the rock masses.

Conclusion.—The author deduces from the three classes of observations a general mean thermic gradient of 48 feet per degree F., but he considers this only an approximation to the true normal gradient, and that the readings of the coal-mines and artesian-well experiments are, owing to the causes he enumerates, still too high. He also discusses the question whether or not the gradient changes with the depth. His own reduction of the observations gave no result, but he points out that in all probability the circulation of water arising from the extreme tension of its vapour is stayed at a certain depth ; while it has been shown experimentally that the conductivity of iron diminishes rapidly as the temperature increases, and this may possibly in a different degree apply to rocks. If, therefore, there is any change, these indications would be in favour of a more rapid gradient.

Taking all these conditions into consideration, the author inquires whether a gradient of 45 feet per degree would not be nearer the true normal than even the one of 48 feet obtained by the observations.

Linnean Society, February 19.—Prof. P. Martin Duncan, F.R.S., Vice-President, in the chair.—The Rev. L. Martial Klein was elected a Fellow.—Mr. Thiselton Dyer exhibited and made remarks on specimens of the peculiar Chinese "square bamboo" (*Bambusa quadrangularis*, Fenzi), and of articles made from the so-called "hairy bamboo" (probably *Dendrocalamus latiflorus*, Munro), sent from Wenchow to the Kew Museum by Dr. Macgowan.—Mr. T. Christy afterwards drew attention to silk fibres received from Auckland, New Zealand.—An abstract of Part iii. of the Rev. A. Eaton's monograph on the Mayflies (Ephemeridæ) was read by the Secretary. In this, the fourth series of group 2 of the genera are dealt with. Among representatives of Section 9, *Cloën* is distinguished by absence of hind wings, *Callibætis* by costal projection and cross-veinlets of its broad obtuse hind wings, *Baetis* by small or absence of costal projection and deficiency of cross veinlets, and *Centroptilum* by extreme narrowness of hind wings and slenderness of costal projection. The distinctive characteristics of sections 10 and 11 of the genera are also taken into consideration, and full descriptions of many new species given.—Then followed notes on the European and North American mosses of the genus *Fissidens*, by Mr. W. Mitten. Referring to the more recent important contributions of Dr. Braithwaite's British Moss-Flora, and Messrs. Lesquereux and James's North American Mosses, and taking into account definitions of older writers, such as Dillenius, Hedwig, Swartz, and others, Mr. Mitten endeavours to arrange the entire group afresh, partly in a tabular form, and afterwards supplementing this by notes on the individual species.—A paper was read by Prof. P. M. Duncan on the anatomy of the Ambulacra of the recent Diadematidæ. The author described the arrangement of the compound plates of the genera of *Diadema*, *Echinothrix*, *Centrostephanus*, *Atropyga*, *Micropyga*, and *Aspidadiadema*. The first three genera have triplets, consisting of primaries, the adoral and aboral plates being low and broad, and the second, or central plate, being a large primary. Nea, the peristome there is deformity of this typical arrangement and in *Echinothrix* a demiplate may enter, but it is never the second plate. In *Astropyga* the triplets are arranged so that the

majority are on the *Diadema*-type, and the exceptions were recorded. The structure of the triplets of *Micropyga* is unique, and the arrangements, leaving out the position of the pores, is somewhat like that of *Cœlopleurus*. *Aspidodiadema*, as has been explained by A. Agassiz, is like *Cidaris* in its ambulacra.

Mathematical Society, February 12.—J. W. L. Glaisher, F.R.S., President, in the chair.—Miss Emily Perrin, Ladies College, Cheltenham, was elected a Member, and Mr. J. Griffiths was admitted into the Society.—Mr. Tucker read the following papers:—"Sur les Figures semblablement Variables," by Prof. J. Neuberg; on the extension of Ivory's and Jacobi's distance-correspondences for quadric surfaces, by Prof. J. Larmor; and some properties of a quadrilateral in a circle the rectangles under whose opposite sides are equal, by R. Tucker. Messrs. Jenkins and S. Roberts spoke on the subject of the first paper. A clear idea of Mr. Tucker's communication will be obtained by drawing a figure for the following particular case:—Take a quadrilateral, $ABCD$, in a circle, with its angles $A, B = 58°, 112°$ respectively, and AB (the unit of length) equal the side (in this case) of the inscribed square. Let $BC = \lambda$, $CD = \mu$, $DA = \nu$; then if two sets of lines drawn in the same *senses* with the respective sides from the two ends make with those sides (in the particular case) angles of 38°, these lines will intersect in two sets of 4 lines in P, P' (analogous to the Brocard points of a triangle), and in four sets of 2 lines in F, G, H, K. The quantities λ, μ, ν, are so related that $\lambda\nu = \mu$, hence we see that all such quadrilaterals have the rectangles under their opposite sides equal. The six points lie on a circle which also passes through the circum-centre (O), point of intersection (E) of the diagonals AC, BD, and through the mid-points M, L of those diagonals. In fact, since OE is a diameter of this new circle, the mid-points of any chord of the circum-circle which passes through E lies on the small circle. P, P' are the foci of an ellipse inscribed in $ABCD$. Further properties are $OP = OP'$, $AP.BP.CP.DP = AP'.BP'.CP'.DP'$, and many other metrical and angular relations belong to the above collection of points. If instead of 38° we take ϕ, then ϕ is found by the equation $\text{cosec}^2\phi = \text{cosec}^2 A + \text{cosec}^2 B$. The side AB subtends at an opposite vertex an $\angle\,\theta_1$, such that $\cot\theta_1 = \cot\phi - \cot A - \cot B$, with similar values for the other angles. The circum-radius (R) is found by—

$$2R^2 = (\cot\phi - \cot A)(\cot\phi - \cot B),$$

and that of the small circle (ρ) by

$$2\rho = R\sec\phi\,\sqrt{\cos 2\phi}.$$

Relations connecting the θ set and ϕ with the Brocard angles of the 4 constituent-triangles are easily obtained in a neat form. If through E lines are drawn parallel to the sides cutting them in eight points, these points lie on a circumference which has many properties analogous to those of the "*T.R.*" circle of a triangle. If ρ' is its radius, then $\rho^2 + \rho'^2 = R^2/_2$: the eight points from two equal inscribed quadilaterals similar to the given figure, and whose sides make the same angle ϕ with the given sides.

Geological Society, January 28.—Prof. T. G. Bonney, F.R.S., President, in the chair.—Frederick John Cullis, Henry Dewes, Henry Hutchings French, Jacob Hort Player, and the Hon. Donald A. Smith, were elected Fellows, and Prof. F. Fouqué, of Paris, and Dr. Gustav Lindström, of Stockholm, Foreign Correspondents of the Society.—The President called attention to the great loss the Society had sustained in the sudden and unexpected death of Dr. J. Gwyn Jeffreys, F.R.S., &c., who had been for twenty-one years continuously a Member of the Council, and for fourteen years of that time had performed most valuable services to the Society as Treasurer.—The following communications were read:—The Boulder Clays of Lincolnshire: their geographical range and relative age, by A. J. Jukes-Browne, F.G.S. The author commenced by referring to the late Mr. Searles V. Wood's papers on the glacial beds of Yorkshire and Lincolnshire, and stated, as the result of his own investigations, that two distinct types of boulder clay occur in Lincolnshire: (1) the gray or blue clay; (2) the red and brown clays, the former undoubtedly an extension of the upper or chalky boulder clay of Rutland and East Anglia, while the second includes the purple and Hessle clays of Mr. S. V. Wood. These two types of boulder clay are very rarely in contact with each other. The brown boulder clays of East Lincolnshire rest upon a broad plain of chalk, which appears to terminate westward in a concealed line of cliff, this cliff-line coinciding with the strike of the slope which descends from the chalk wolds to the boulder clay plateau by which they are bordered. The present boundary line of the boulder clay runs along this slope for long distances, though in many places the clay has surmounted the slope and caps the hills to the west of it. From Louth the main mass of the "brown clay" is bounded by a line drawn through Wyham, Hawerby, Laceby, and Brocklesby to Barrow and Barton on Humber, sweeping around the north end of the Lincolnshire wolds and occurring on both sides of the Humber. Previously to the author's inspection of this district no purple or Hessle clay had been discovered west of South Ferriby, and these clays were supposed to be entirely absent on the western side of the wolds. The officers of the Survey have, however, mapped several tracts of such clay in the valley of Ancholme. It occupies the surface at Horkstow, Winterton Holme, Winterton, and Winteringham. It probably underlies the alluvium of the Ancholme near and south of these places, and occurs again at higher levels in the neighbourhood of Brigg. South of Brigg it has been seen at low levels on either side of the valley of the Ancholme, as far as Bishop's Bridge near Glentham. Beyond this point it was not traceable in the Ancholme valley, but south of Market Rasen patches of reddish-brown clay, mottled with gray, and containing small flints and pebbles of chalk, occur, and cap the low ridges separating the valleys of the brooks. Another tract of boulder clay, which the author considers to belong to the same series, occupies the western border of the fenland south-east of Lincoln, what is left of it forming a ridge which runs southward for many miles. It passes eastward beneath the fen deposits; and similar mottled clay was seen in the excavations for the Boston Docks beneath about twenty feet of fen clays, &c., and resting upon blue boulder clay of the "chalky" type. Besides this section at Boston, there are very few places where the two types of clay are in contact, or so near as to afford any evidence as to their relative age. Near East and West Real, and again near Louth, the "brown clays" are banked against the slopes of hills which are capped with the "chalky clay." The same is the case also near Brigg, where the country seems to have been originally covered by a sheet of the chalky clay, through which valleys were eroded into the Jurassic clays, and the brown (Hessle) clay is found only in these valleys. The author concludes, therefore, that the "Brown-clay series" is of much newer date than the "Blue and Grey series." In conclusion the author summed up the inferences drawn in the paper, correlated the Basement clay of Holderness with the Chalky clay of Lincolnshire, and suggested that the Purple clay may be confined to the east side of the wolds. The classification he would propose is therefore as follows:—

	Lincolnshire.	Yorkshire.
Newer Glacial.	Hessle clay.	Hessle and upper red clay of coast.
	Purple clay.	Purple clay.
Older Glacial =	Chalky clay.	Basement clay.

—On the geology of the Rio Tinto Mines, with some general remarks on the pyritic region of the Sierra Morena, by J. H. Collins, F.G.S. After briefly describing the geographical position of the Rio Tinto mines and the occurrence at the same of pyritous ores amongst slates and schists which abut against gneissose rocks to the north, and pass under Tertiary beds to the southward, the author proceeded to consider the general characters and associations of the pyrites-deposits, and then gave a general account of the Rio Tinto district. The slates were described, and the fossil evidence recapitulated upon which an Upper Devonian age had been assigned to them. Analyses were furnished to show the changes due to weathering and to infiltration. The various intrusive rocks (syenite, diabase, and porphyries) occurring in the schists were described, and analyses of them given. The sedimentary iron ores and their composition were next noticed, and the author ascribed their formation to deposition in lakes. The masses of pyrites which furnish the principal ores of Rio Tinto were then described, their mode of occurrence in fissures between dissimilar rocks explained, and their formation discussed. The different kinds of ore obtained from the mines were noticed in detail, and several analyses added, giving samples both of the mixed ores and of the pure minerals. The manganese lodes were next described, and shown to be parallel to the pyrites fissures, and frequently to be only branches of the latter. A summary of the author's conclusions as to the stratigraphy of the district, the ore deposits, and the surface-geology was appended.—On some new or imperfectly known Madreporia from the Great Oolite of the counties of Oxford, Gloucester, and Somerset, by R. F. Tomes, F.G.S.

Physical Society, February 14.—Annual General Meeting.—Prof. Guthrie, President, in the chair.—Prof. G. Fuller was elected a Member of the Society.—The President then read the Report of the Council, in which the Society was congratulated upon the number of original communications read—forty-three during the past year. Among the works undertaken by the Society may be mentioned the publication of the first volume of "Joule's Scientific Works"; a second volume, containing accounts of researches conducted by Mr. Joule in conjunction with other scientific men, would be published shortly.—The Treasurer presented a highly satisfactory report.—The Council for the ensuing year was then elected, the result of the election being as follows:—President: Prof. F. Guthrie, Ph.D., F.R.S.; Vice-Presidents (who have filled the office of President): Dr. J. H. Gladstone, Prof. G. C. Foster, Prof. W. G. Adams, Sir W. Thomson, Prof. R. B. Clifton; Vice-Presidents: Prof. W. E. Ayrton, Shelford Bidwell, Lord Rayleigh, Prof. W. C. Roberts; Secretaries: Prof. A. W. Reinold and Walter Baily; Treasurer: Dr. E. Atkinson; Demonstrator: Prof. F. Guthrie; other Members of Council: C. Vernon Boys, C. W. Cooke, Prof. G. Forbes, Prof. F. Fuller, R. T. Glazebrook, Dr. J. Hopkinson, Prof. H. McCleod, Prof. J. Perry, Prof. J. H Poynting, Prof. S. P. Thompson; Honorary Member: Prof. M. E. Mascart.—The customary votes of thanks to the Committee of the Council of Education and to the President, Secretaries, and other officers having been passed, the meeting resolved itself into an ordinary meeting of the Society.—Miss Marks described a new line and area divider. This instrument consists of a hinged rule with a firm joint. The inside edge of each limb is bevelled, and presents a straight edge. One limb is divided on both edges into a number of equal parts, and is fitted by a groove on its outer edge to a plain rule, along which it can slide. To divide a line into a given number of equal parts, the hinged rule is slid along the plain rule till the nth division from the joint is opposite a fixed mark on the plain rule; it is then placed on the paper so that the nth division on the graduated straight edge coincides with one end of the given line, and then opened till the straight edge on the inner edge of the other limb passes through the other extremity. The plain rule is then pressed firmly down and the hinged rule slid along it. As each division of the graduated edge passes the fixed mark, the intersection of the moving edge with the given line is marked, and thus the line is divided into n equal parts. The instrument may be used in this way to draw any given number of equidistant parallel lines between two given points. It may be conveniently used in working out indicator diagrams and measuring areas.—Mr. Walter Baily described certain improvements made in his integrating anemometer, which has been previously described. The improvements consist in the substitution of mechanical counters for electrical ones, as it was found, in the recent observations with the instrument at Kew, that the extra friction of the "contact" was sometimes sufficient to stop the motion. The mechanical counters were found to work satisfactorily in every respect.—Prof. Guthrie showed some specimens exhibiting the similarity of fracture of Canada balsam and glass. The glass had been cracked by heating a metal ring to which it was attached; the Canada balsam had been overheated in a small dish and allowed to cool.

Zoological Society, February 17.—Osbert Salvin, F.R.S., Vice-President, in the chair.—Mr. F. E. Beddard, F.Z.S., read a paper upon the structure of the Cuckoos (Cuculidæ), and pointed out the differences in the pterylosis and the structure of the syrinx in the various forms which he had examined. It was proposed to divide the family into three sub-families: Cuculinæ, Phœnicophainæ, and Centropodinæ.—Mr. F. E. Beddard read a paper upon the heart of *Apteryx*, and called attention to the variations in the condition of the right auriculo-ventricular valve observed in different individuals of this bird.—A communication was read from Mr. M. Jacoby, containing the first part of an account of the Phytophagous Coleoptera obtained by Mr. George Lewis during his second journey in Japan, from February, 1880, to September, 1881.

Geologists' Association, February 6.—W. H. Hudleston, F.R.S., in the chair.—The annual meeting was held at University College.—The following Officers were elected for the ensuing year:—President: W. Topley, F.G.S., Assoc.Inst.C.E.; Vice-Presidents: Prof. J. F. Blake, M.A., F.G.S., T. V. Holmes, F.G.S., W. H. Hudleston, F.R.S., F.G.S., F.C.S., Henry Hicks, M.D., M.R.C.S., F.G.S.; Treasurer: J. Hopkinson, F.G.S., F.L.S.; Secretary: John Foulerton, M.D., F.G.S.; Editor: Prof. G. S. Boulger, F.L.S., F.G.S.; Librarian: J. Bradford, F.G.S.; Council: J. Logan Lobley, F.G.S., F.R.G.S., Ed. Litchfield, A. C. Maybury, F.G.S., J. Love, F.G.S., F.R.A.S., W. H. Bartlett, F.G.S., T. Davis, F.G.S., J. J. H. Teall, F.G.S., R. Meldola, F.C.S., J. Slade, F.G.S., J. S. Gardner, F.G.S., Prof. T. Rupert Jones, F.R.S., B. B. Woodward, F.G.S.—Prof. T. R. Jones, F.R.S., gave an address on Foraminifera, recent and fossil, and Mr. F. W. Rudler one on some points in connection with volcanic action; both were illustrated by lantern views exhibited by Mr. G. Smith.—Many instructive objects were exhibited, amongst them a series of Palæolithic implements from France, Spain, and England, by Dr. J. Evans. F.R.S.

EDINBURGH

Royal Society, February 2.—Mr. Thomas Stevenson, President, in the chair.—The President delivered an address, in which he discussed the erection of training-walls at the mouth of the Mersey. He would strongly condemn such a procedure, asserting that the inevitable result would be the silting up of the approaches to Liverpool.—Prof. Tait submitted a paper on condensation and evaporation. He pointed out that the present mode of treating the conditions of a liquid in presence of its vapour were not rigorous, inasmuch as the pressure is undoubtedly different in the two parts, while in the surface-layer between them there is a complex form of stress. If attention be confined to the isothermals of the interior parts of a liquid, or of its vapour, the present method will apply rigorously. With this proviso the isothermals under the critical point consist of two parts separated by an asymptote—one belonging to the liquid, the other to the vapour. This accords with the fact that liquids can be subjected to hydrostatic tension, and that Aitken has shown that true vapour cannot be condensed without a nucleus.—Mr. John Rattray, of the Granton Marine Station, communicated a note on Ectocarpus.—The Rev. J. M. Macdonald exhibited some specimens from Philadelphia which had the appearance of large vegetable fossils. Mr. John Murray and Prof. Duns pronounced them to be merely inorganic accretions around reeds.

Mathematical Society, February 13.—Mr. A. J. G. Barclay, President, in the chair.—Prof. Tait communicated a note on a plane strain, which was read by Mr. W. Peddie; Dr. Muir gave an account of a paper by Mr. P. Alexander on Boole's proof of Fourier's double integral theorem, and afterwards enunciated several theorems of his own on the arbelos; Mr. Peddie discussed reflected rainbows; Mr. Allardice gave a note on spherical geometry: and Mr. A. Y. Fraser made some remarks on a problem in plane geometry.

CAMBRIDGE

Philosophical Society, February 2.—Prof. Foster, President, in the chair.—Prof. C. S. Roy, M.A., was elected a Fellow.—The following communications were made:—On the Zeta-function in elliptic functions, by Mr. J. W. L. Glaisher.—On a certain atomic hypothesis, by Prof. K. Pearson. Communicated by Mr. H. T. Stearn.—On a Young's eriometer, by Mr. R. T. Glazebrook.

PARIS

Academy of Sciences, February 16.—M. Bouley, President, in the chair.—On the inaccuracies committed in the employment of the usual formulas in the reduction of the polar stars and in determining the astronomic collimation. The correct terms required to remove these errors. Method of observing the polar stars at any meridian distance, by M. Lœwy.—Description of the nervous system of *Ancylus fluviatilis*, by M. H. de Lacaze-Duthiers.—On the order of appearance of the first vessels in the leaves of the cruciferæ; third part, *Crambe maritima*, *juncea*, and *cordifolia*, by M. A. Trécul.—Experiments on some phenomena of the movement of water in an apparatus employed to raise the liquid by means of a mechanical fall without piston or lifting valve, by M. A. de Caligny.—On the resistance of keels in connection with the velocities of 20 and 21 knots an hour recently obtained without special extra motor power, by M. A. Ledieu.—On the oïdium, *Phoma vitis*, mildew (*Peronospora viticola*), and some other cryptogamic diseases prevalent for some years past in the European vineyards, by M. H. Marès.—On the density and figure of the earth, by Gen. L. F. Menabrea. The author's researches tend to confirm the anticipations of Newton that the mean density of the earth would be found to lie between five and six times that of water.—On the development of the vascular apparatus, and of the reproductive organs

in the comatulæ, by M. Edm. Perrier.—Extraction of the green colouring matter of leaves; definite combinations formed by chlorophyll, by M. Er. Guignet. — On some theorems in algebra, by M. Stieltjes.—On the heating power of coal-gas in various states of dilution, by M. A. Witz. From his experiments the author infers that the complete combustion of gas requires a dilution of over six volumes of air, the effect of the dilution thus being the reverse of what might be supposed *à priori*.—On the laws of solution, by M. H. Le Chatelier. From his researches the author concludes that solubility increases with the temperature for bodies whose solution absorbs heat, decreases for those that liberate heat, and remains unchanged when the heat of solution is null.—On the solution of the carbonate of magnesia by carbonic acid, second note, by M. R. Engel.—On a crystallised hydrate of phosphoric acid, by M. A. Joly.—Note on the cellular structure of cast steel, by MM. Osmond and Werth.—On glycol, its preparation and solidification, by M. G. Bouchardet. A very pure preparation of glycol, obtained by a solution of carbonate of potassa acting on the bromide of ethylene, was found to boil at 198° C., and to solidify at temperatures varying from $-11°\cdot5$ to $-25°$.—Note on monochlorhydric glycol, by M. G. Bouchardat.—Action of the diastase of malt on natural starch, by M. L. Brasse.—On the rotatory power of the solutions of cellulose in Schweizer's liquid, by M. Alb. Levallois.—Observations regarding the organisms to which fermentation is due; claim of priority of discovery in connection with some remarks of M. Pasteur on a recent note of M. Duclaux, by M. A. Béchamp.—Note on the anatomical structure and classification of *Halia priamus* (Risso), by M. J. Poirier.—On the anatomy of the brachiopods of the genus Crania, by M. Joubin.—On the nervous system of a Fissurella (*F. alternata*), by M. L. Boutan.—On the origin of the metalliferous ores existing on the periphery of the central plateau of France, and especially in the Cevenne highlands, by M. Dieulafait.—On the results of M. Sokoloff's studies on the formation of sandy dunes in Central Asia, by M. Venukoff.

BERLIN

Physiological Society, January 21.—Dr. von Monakow, referring to his anatomical investigations of the brain, communicated an account of those relating to the central origin of the optic nerve. He had enucleated on one or both sides the bulbus in young rabbits and cats, and, after an interval of some months, examined the changes which had set in as a result of that violence done to the brain. In each case he found regular ascending atrophy capable of being traced up to the origin of the nerves. By this means he had been able to recognise as central original spots of the nervi optici the corpus geniculatum externum, the pulvinar and the anterior corpora quadrigemina. The corpus geniculatum and the pulvinar consisted of large multipolar cells, between which lay a gray medullary substance, which, on being coloured with carmine, showed a particularly strong tinge. After the enucleation, atrophy of the gray medullary substance was observed in both, while the cells remained altogether intact. On colouring with carmine, the somewhat shrunken organs appeared much paler than in the normal state. In the corpora quadrigemina five different layers of small and large cells and fibrous bands were distinguished. Of these the three innermost layers lying towards the ventricle remained intact, while the two exterior cellular layers were atrophied or were altogether wanting. The degeneration and disturbance of growth after the enucleation of the bulbus had not, however, extended beyond these primary centres of the optic nerve. Dr. von Monakow had, furthermore, removed particular parts of the cerebral cortex lying within Munk's sphere of vision, and the degeneration and atrophy which succeeded this injury, and, extended peripherically, could be followed through Gratiolet's fibres on to the three centres of optic nerves above mentioned, the corpus geniculatum externum, the pulvinar and the anterior corpora quadrigemina (*Vierhügeln*), and beyond these centres as far as the tractus opticus and the optic nerves. It was an interesting fact that after the injury of the cerebral cortex the degeneration of the three centres of optic nerves was of a different character from that which set in after the peripherical enucleation. The corpus geniculatum and the pulvinar were now altered in such a manner that it was mainly the cells which either showed degeneration or were entirely wanting. In the anterior corpora quadrigemina, likewise, it was other layers—namely, the third medullary layer and the larger cells—which were overtaken by degeneration. The speaker had had the opportunity, in making a dissection, of substantiating on a man who had long been suffering from diseased retina, that the degeneration in the case of man propagated itself centrally—towards the three centres before-mentioned—just as much as in the case of the rabbits operated on.—Dr. Weyl spoke on casein, which took quite an exceptional place among albuminous bodies. According to the most recent researches albuminous bodies contained only O, H, N, C, and S, but no phosphorus, and might be divided into (1) albumins or albuminous bodies soluble in water; (2) globulins, insoluble in water, but soluble in solution of common salt; (3) proteins, soluble neither in water nor solution of common salt, but in diluted alkalis. Finally, a fourth group of albuminous bodies was formed by such as were soluble in none of those reagents, but, except in this one characteristic, had no affinity to each other, such as fibrin, amyloid, casein, &c. Casein had hitherto been identified only in milk. It was an albuminous body, because under the agency of diluted muriatic acid and pepsin it yielded a pepton, and, besides, precipitated an insoluble substance, which must be classed among the nucleins. Casein contained phosphorus, and so was distinguished from all other albuminous bodies. In order to the demonstration of casein and its quantitative determination in milk, Dr. Weyl had, in conjunction with Dr. Frentzel, adopted a new and less detailed process than that of Prof. Hoppe-Seyler so universally introduced into practice. This new process consisted in diluting the milk threefold and reducing it with highly diluted sulphuric acid (1 : 1000). Thereupon a flaky precipitate at once segregated itself, which could be filtered off and weighed. The precision of this method was equal to that of Hoppe-Seyler's, and by means of it Dr. Weyl and Dr. Frentzel had begun to study quantitatively the transformation of casein into pepton and nuclein. The speaker hoped to be able soon to make communications regarding the result of this investigation.—Dr. Rossel had examined the nuclein of the yolk, in order to test the assertion of Mr. Michat that it resembled the nuclein of cell-nuclei, an assertion which lent a chemical support to the view of Prof. His that the granules of the yolk entered as organic elements towards the upbuilding of the embryo, and formed the cell-nuclei. Dr. Rossel had isolated the nuclein of the yolk of hen-eggs, and, on examining it, had found it essentially different from the nuclein of cell-nuclei. While this latter contained the highly nitrogenous organic bases guanin and hypoxanthin, none of these bases were found in the nuclein of the yolk. The nuclein of the yolk was, therefore, essentially different from that of the cell-nuclei, and under the demonstration of this difference the support which, from the chemical side, had been afforded to the view of the transference of granular formations of the yolk into cell-nuclei, fell away.

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